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ORIGINAL ARTICLES.

I.—On *Didymograptus*, *Tetragraptus*, and *Phyllograptus*.

By Dr. Gerhard Holm,
Palæontologist to the Geological Survey of Sweden, Stockholm.

Translated from the original in Geol. Fören. i Stockholm Förhandl., Bd. XVII, Häft iii, No. 164, 1895, pp. 319–359. By G. L. Elles and E. M. R. Wood, Newnham College, Cambridge.

(PLATES XIII AND XIV.)[1]

THE knowledge of the internal structure and development of the different forms of Graptolites is very incomplete, owing to the unfavourable method of their preservation, Graptolites, in almost all cases, being found embedded in shale.

For more than ten years, however, I have been acquainted with Graptolites which, exceptional though it may be, are embedded in pure limestone, and are usually preserved in full relief; not unfrequently also the chitinous substance is so well preserved, that the polypary can be set free by dissolving the limestone with acid.

The first Graptolites which, by this means, have been isolated and studied by me, were *Climacograptus Kuckersianus*, nov. sp., Holm, from a specimen collected by G. Linnarsson, near Kuckers, in Estland, and now the property of the Geological Institution at Upsala, and *Retiolites Geinitzianus*, Barr., in a limestone nodule from Motala. I have since described the internal structure of the latter, principally with the aid of the above-mentioned specimen.[2] In the same paper I have given an account of an improved method of dissolving the matrix, by means of which, for example, such fragile objects as the connecting parts of the reticulate polypary of *Dictyonema cervicorne*, Holm, and the fork-shaped, apertural spines of the thecæ, can be isolated.

Since the above-mentioned time, I have constantly collected as much material as I could obtain suitable for treatment by this method, for the purpose of a closer study of the structure, development, and affinities of Graptolites; and I have now isolated many thousands of specimens belonging to a great many forms of different groups of Graptolites.

[1] The plates and page-illustration will appear with the second part of this paper next month.

[2] Bihang K. Vet. Akad. Handl., Bd. 16, Afd. iv, No. 7, 1890.

In this paper an account is given of the development and structure of the polypary in the genera *Didymograptus*, *Tetragraptus*, and *Phyllograptus*. This is based exclusively on the excellent material, which I have succeeded in collecting during many years, from the grey glauconitic Vaginatenkalk of various localities in N. Öland. This limestone contains but few Graptolites, but they are in a condition well fitted for isolation.

According to the general current opinion, which seems to have been founded on Lapworth's description of the development of the graptolitic polypary in "On an Improved Classification of the Rhabdophora," the development in the bilateral and the diprionidian forms takes place from two buds, which arise directly on the two sides of the sicula. Certainly Lapworth himself has since changed his former opinion, and with his usual penetration, on the ground of the structure of the genus *Dimorphograptus*, has stated as theoretically more probable that the sicula in all the "Graptoloidea" gives rise to only one bud: this opinion, however, has been disregarded. Quite recently, first Törnquist and then almost simultaneously Wiman, who had at their disposal far better material than Lapworth, conclusively showed that in the Diplograptidæ only one bud is developed from the sicula, and that both rows of thecæ are derived from this.

In the following pages it will be shown that, in the main, a complete conformity exists in the first stages of development of the polypary, both in the genera *Didymograptus*, *Tetragraptus*, and *Phyllograptus* and in the family Diplograptidæ. It may, therefore, be considered as in the highest degree probable that the earlier development of the proximal part (the three first thecæ) in all the bilateral or diprionidian forms of Graptolites is, in the main, the same, and has taken place through the formation of only one bud on one side of the sicula—or first theca, as I believe it is—which side is always the same in relation to the later development of the polypary. From this bud thereafter is developed partly the second theca; partly the canal—"connecting canal"—which connects both halves of the polypary and which in the first place gives origin to the third theca (=first theca on opposite side of sicula); and partly also the common canal, which connects the second theca with the succeeding ones.

In the family of the Monograptidæ, which must be considered as degenerate from the Diplograptidæ, the "connecting" canal, which otherwise bends over to the opposite side of the sicula, and from which the thecal row of that side ought to come, is not developed, but is entirely absent. From the bud on the sicula, therefore, only the second theca is developed, together with the common canal for the single or main branch of the polypary. From the sameness of the structure of the proximal part arises the intimate relationship between the Graptolite forms, separated in time from one another; and this also determines how the younger forms arise from the older through a gradually increasing simplification at first of the distal part, and then, as in the Monograptidæ, of the proximal part of the polypary.

General review of the structure of the proximal part of the polypary (Terminology). — For the sake of conciseness and clearness in description, and an easier comparison between different forms, and also for greater precision in the terms employed, many of which have been only lately introduced, there follows here a general description of the proximal end in the genera described below. The terms have been adapted as far as possible so that they can be also used for the other groups of Graptolites which are provided with a sicula.

The *sicula*, which has a cone-like form, is defined by Lapworth [1] as "The chitinous covering of the free zooid 'germ' or embryo in the Graptoloidea." As Wiman has shown in *Diplograptus*, sp., and in *M. dubius*, Suess, the sicula is composed of two essentially different parts, the proximal and the distal, distinguished by the different nature of the periderm. In the proximal or more pointed end of the sicula, or, as it may perhaps be better termed, the *initial part*, the periderm-wall is very thin and pellucid, and provided with longitudinal branching and anastomosing swellings or striæ, which disappear near the boundary with the distal part, but which, uniting together at the apex of the sicula, form the projecting virgula. In the wider or distal (or perhaps better termed *apertural*) portion of the sicula, the periderm is thicker and less pellucid, and it is provided with lines of growth like those of the rest of the polypary. In opposition to Wiman, I believe that the more pointed end of the sicula, which has no growth-lines, exactly corresponds to the original "chitinous covering of the free zooid germ or embryo," and thus is the "initial part" from which the "*apertural part*" is developed which everywhere shows normal lines of growth.

The *apertural part* of the sicula, as far as can be judged, has the same function as a theca, and might therefore be justly considered as the first theca of the polypary (see fig. 1). The old name "sicula" is nevertheless a convenient and significant one, and is here retained. Even if there be some doubt whether the whole apertural part of the sicula should be regarded as the first theca, it cannot at least be extended to that part of the same which lies below the passage opening into the second theca. It is evident from the direction of the lines of growth at this spot in *Diplograptus*, sp., and *M. dubius*, Suess, that this connection with the growth of the sicula first appears as a notch in the apertural margin, which afterwards by further outward growth of the edge of the sicula is changed into a foramen closed all round. The common canal, by which all the cells of the polypary are connected with one another—whether the different individuals have been developed by budding from the cœnosarc, or whether they are developed from and connected with the one immediately preceding—(the common canal) must therefore be considered to begin already in the sicula, even if it has been convenient, for the sake of description, to distinguish one part as the "connecting" canal. There does not exist any important morphological difference whatever between the part of the sicula lying below the passage opening to the second theca and the remaining thecæ.

[1] Quart. Journ. Geol. Soc., vol. xxxi, 1875, p. 639.

In the bilateral and diprionidian forms the sicula always occupies a side position, more or less oblique to a median plane between the second and third thecæ, dividing the polypary symmetrically into two similar halves. This arises in consequence of the fact that the "connecting" canal between the second and third thecæ, and thus between the two side halves of the polypary, proceeds along one side of the sicula, and more or less completely embraces it. The sicula occupies, therefore, on one side of the polypary a more or less superficial position, so that it is here exposed, either completely, as in *Didymograptus*, *Tetragraptus*, and *Phyllograptus*, or for a longer or shorter space nearest the aperture, whilst the rest of it is embedded in the polypary, as in *Diplograptus* and *Climacograptus*.

This side of the polypary is called in the sequel the *sicula side*; the opposite side, which is occupied by the "connecting" canal between the two halves of the polypary (second and third thecæ) is the *anti-sicula side*.

The sicula here is either completely obscured—for example, in *Diplograptus*, *Climacograptus*, and *Phyllograptus*, or visible only near the aperture and mucro, as in such bilaterally developed monoprionidian forms as *Didymograptus* and *Tetragraptus*. In the former the sicula on this side is completely embedded in the polypary, and is concealed not only by the "connecting" canal but also by the oldest theca and the common canal. In the latter, again, the connecting canal forms only a broader or narrower band across the sicula.

The sicula side in descriptions is termed the *anterior*, that is the side turned towards the spectator, which, when pointing out the different position of the parts, is the usual position of the polypary. For the same reason the anti-sicula side is termed *posterior*. In this I agree with Törnquist—"Structure of Diprionidæ"—who uses the terms "obverse aspect" and "reverse aspect" in the same sense.

The *second* or *left theca*, as it may be called from its position, or more correctly the bud from which it is afterwards formed, always grows out from the left side, and lies along this near the aperture of the sicula (*Didymograptus* and *Tetragraptus*) or ends against it, in which latter case it makes a bend to the left (*Diplograptus* and *Phyllograptus*). The apertures of the sicula and left theca are always turned in opposite directions—that of the sicula to the right, and in the same direction as the thecæ of the rows or branches lying on the right of the sicula; the left-hand theca towards the left, and in the same direction as the remaining thecæ on the left half of the polypary. The direction of the boundary between the sicula and left theca determines, therefore, the direction of the polypary.

The position of the passage opening from the sicula, and thus the place from which the bud grows out, varies. In *Phyllograptus* it is situated quite close to the apex of the sicula, in *Tetragraptus Bigsbyi*, Hall, probably slightly lower down, in *Didymograptus minutus*, Törnq., somewhat below the middle of the sicula, in *Didymograptus gracilis*, Törnq. Mut., still nearer the aperture; but in *Didymograptus gibberulus*, Nich., the position is almost the

same as in *Phyllograptus*, if one disregards the long thread-like initial part of the sicula.

The connecting canal.—From the bud developed on the sicula, there arises almost simultaneously with the left theca the common canal for the left half of the polypary and the connecting canal, which crosses the dorsal side of the sicula and gives origin to the third (or, better, the right) theca lying on the right side of the polypary, and also the common canal for the right half of the polypary. This is particularly clear and conspicuous in *D. minutus*, Törnq. Mut., and the same thing also occurs in *Tetragraptus* and *Phyllograptus* (see Figures). By the connecting canal, therefore, we understand in the sequel the canal surrounded on the outside by a special wall, which crosses the sicula on the dorsal side of the polypary, and forms the connection between the two halves of it.

By the development of the three thecæ just described, namely the sicula, the left and right thecæ (primordial thecæ), there originates the primitive form, from which by bifurcation and growth in different directions a further increase of the polypary takes place. This is, in its general characters, in complete agreement in the representative Graptolites referred to, and therefore probably similar in all Graptolites where degeneracy has not taken place.

The genus *Didymograptus*, from the left theca of which there arises only one left branch, and similarly from the right only one right branch, constitutes a further stage of growth of the primitive form. This primitive form or state of development we can therefore term the *Didymograptus*-stage. If no other facts of a different nature are known, it is impossible to assign an individual to its proper genus when it is only known in this or in an earlier stage of development. This is shown in Figures 13–16, Pl. XIV, which represents a young form of *Tetragraptus Bigsbyi*, Hall, in the *Didymograptus*-stage. An account of the further bifurcation belongs to the description of the different genera.

The virgula.—The virgula is wanting in the genera here described; it has, moreover, never been observed by me in any forms belonging to the family Dichograptidæ. On grounds which will be shown later on, a virgula corresponding to that in *Diplograptus* and *Monograptus* cannot occur in the Dichograptidæ. The same is the case in the Dictyograptidæ, Tullb., in forms of Nemagraptidæ, and in the genus *Dicellograptus* of family Dicranograptidæ. A virgula seems never to have been described or figured in any allied form. In the family Diplograptidæ a virgula has been observed in a great many cases, and probably therefore occurs generally (or always?). In the family Monograptidæ it at least is often found, and in many cases has been figured. I have not yet had an opportunity of observing whether both the chitinous threads, which in the Retiolitidæ (*Retiolites* and *Stomatograptus*) have been called the straight and zigzag virgulas, correspond with and have the same mode of origin as the virgula in *Diplograptus*. That a virgula corresponding to that in *Diplograptus* and *Monograptus* cannot occur in the families

and genera first enumerated, results from the mode of origin of the same, as a prolongation of the initial part of the sicula.

Törnquist (Siljansområdets Grapt., i, p. 16) has already pointed out, in the descriptions of *Didymograptus minutus*, Törnq., that the thread-like prolongation of the sicula most nearly corresponds with the virgula in the genera *Diplograptus* and *Monograptus*. That this must be the case is evident from Wiman's description of the origin of the virgula from the sicula in *Diplograptus*. Whilst it has been previously supposed that the virgula extended lengthways along one side of the sicula, and therefore in the developed polypary often formed a free, thread-like prolongation of the proximal as well as the distal end of the same, Wiman has shown that the virgula of the sicula, in the sense which he, in conformity with former authors, gives to it, is not continuous, but consists of two separate parts. The one of these, which forms the beginning of the true virgula, stretching throughout the whole of the polypary, originates first near the apex of the sicula as a prolongation of the same, and in *Diplograptus* is embraced and comprised within it by reason of the direction of the development of the polypary. It is this, which corresponds to the thread-like prolongation of the apex of the sicula mentioned above, which occurs in certain species of *Didymograptus*, e.g. *D. gibberulus*, Nich., where it extends beyond the length of the branches.

Since the apex of the sicula is free, as is the case in the above-cited Graptolite forms, a virgula cannot consequently occur embedded in the dorsal side of the branches. It may be observed that in genera in which the sicula is embedded in the polypary a virgula need not of necessity be present.

The presence or the absence of a virgula depends on whether the initial end of the sicula is protracted or not. Thus, for example, the virgula is absent in *Phyllograptus*, and such has hitherto never been observed in any allied species; and in more than a hundred of my isolated or otherwise prepared examples of *Phyllograptus angustifolius*, Hall, in which the internal structure can be studied, no trace of a virgula has ever been observed.

The second of the forms referred to the virgula by Wiman consists of a cylindrical chitinous thread, which originates as a result of growth within the apertural end of the sicula, at some distance from the initial portion. This later structure stands evidently in no relation whatever to the real virgula, but may be regarded as an apertural spine.

The presence of a virgula has, curiously enough, hitherto been considered as the main character of Graptolites. Thus, for example, Lapworth[1] characterizes the sub-order Rhabdophora as follows: "Hydroida in which the polypary is strengthened by a chitinous filiform virgula." Zittel[2] also emphasizes the presence of a virgula in the sub-order Graptoloidea, and says: "Through the possession of a virgula, Graptolites differ from all other Hydroida." The generally

[1] Geol. Mag., Vol. X, 1873, p. 555.
[2] Handb. der Pal., p. 293.

accepted opinion concerning the presence or absence of a virgula in all Graptolites, although such was never described or expressly mentioned, except in the groups Diplograptidæ, Monograptidæ, and Retiolitidæ, might be explained by the two following circumstances:—One of these is that Barrande only knew of forms with a virgula, with the exception of *Retiolites*, in which genus he certainly had observed the chitinous filament named by later authors "a straight virgula," but referred it to the outer network. He began his chapter on the virgula (solid axis) thus: "Graptolites are always provided with a virgula."[1] Through his authority he might so have influenced later authors that his representations concerning the virgula became a creed. The other circumstance might be that broken branches of Dichograptidæ have long been confused with species of *Monograptus* in which the virgula is really present, hence the absence of the virgula in the former has never been questioned.

How strongly the idea of the presence of a virgula in all true Graptolites has taken root is best shown by the fact that so keen and critical an observer as Brögger[2] was disposed to admit the presence of a virgula in *Dictyonema* and *Bryograptus*, although he had never observed it in these forms.

Genus Didymograptus, M'Coy. Pl. XIV, Figs. 1-3, 7, 8.

Concerning the structure of the proximal part of *Didymograptus*, one finds hardly anything mentioned in literature, and in the figures of its species the proximal part is in the greater number of cases so inconspicuously or indefinitely indicated, and figured only from one side, that no definite conclusions can be drawn as to its structure. One exception is Moberg's description of *Didymograptus gibberulus*, Nich.—"Nyagrapt. från Skåne," p. 339, pl. viii—for which, however, Moberg, in ignorance of the true structure of the *Didymograptus* polypary, and misinterpreting the characters in the incomplete material at his disposal, founded a new genus, *Isograptus*. As will be seen below after the description of the *Didymograptus* polypary, Moberg's description and figures, properly interpreted, show clearly that the proximal part of *Isograptus gibberulus* completely agrees with that of the genus *Didymograptus*. Some observations about the *Didymograptus* polypary are mentioned further by Törnquist (Siljansområdets Grapt., i, p. 15). Concerning the sicula, Törnquist remarks that it seems sometimes as if it "had been divided first by an oblique wall into two parts, and as if each part sent out a separate branch." The figures of the proximal part—figs. 8, 12, and 14—which all show the sicula side, indicate that it is the sicula and left theca joined, together with the walls between them, which are referred to by Törnquist. Törnquist states further that the sicula "keeps its triangular form, and by a conspicuous wall is separated from both stipes," which always arise "at different levels from the sides of the sicula, and seem to have originated by a kind of budding."

[1] Grapt. de Bohème (1850), p. 4.
[2] Die Silur. Etagen, 2 u 3, 1882, p. 37.

This also points to conclusions drawn exclusively from the sicula side. As above mentioned, Törnquist also has observed the thread-like process sometimes arising from the apex of the sicula, and has rightly indicated that the nearest correspondence to this in the genera *Diplograptus* and *Monograptus* is the virgula. The two species of *Didymograptus* which are here described, are both from the grey glauconitic Vaginatenkalk at Hälludden, near Torp in Böda parish, in Öland.

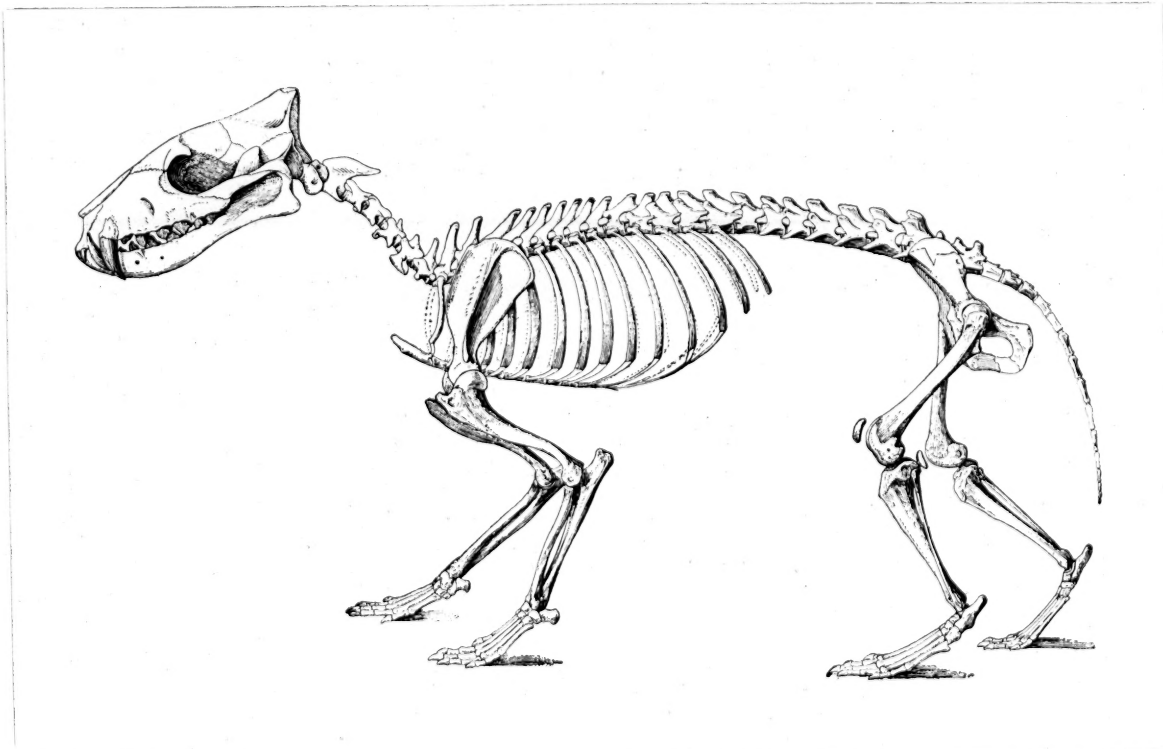
DIDYMOGRAPTUS MINUTUS, Törnq. Mut. Pl. XIII, Figs. 1-3.

The three figures give, even at the first glance, a far clearer picture of the external and internal structure of the proximal part than any description. Fig. 1 shows a cast of the sicula side, with the partitions appearing as dark lines on the rock surface; Figs. 2 and 3 show the anti-sicula side in reflected and transmitted light, so that the internal walls and growth-lines of the periderm become visible, and also a simultaneous picture of the external shape, the course of the growth-lines, the internal divisions, and the connection between all these thus obtained.

The apex (initial part) of the sicula is broken off. All over the remaining part, and the polypary besides, conspicuous and regular lines of growth can be seen. The passage opening for the canal arising from the sicula is in this case situated nearer the apertural margin. This appears in Figs. 1 and 3 as a discontinuity in the wall of the sicula. The canal arising from the sicula exhibits on the anti-sicula side (Fig. 3) a peculiar characteristic and instructive arrangement of growth-lines, showing how from the bud in the left wall, by development in different directions, there is formed simultaneously partly the "connecting" canal, which lies on the dorsal side of the sicula, and partly also the common canal of the left stipe. Again, the left theca seems here to be completely separated from the common canal by a wall which runs with the sicula wall, and, without a break, stretches to the aperture of the theca (interthecal wall between left theca and the succeeding theca). This is confirmed by the lines of growth, which, on different sides of the wall, do not stand in any relation to one another.

On the sicula side (Fig. 1), on the contrary, matters are seen from the cast to be quite different. The canal arising from the sicula is here in free and open connection with the left theca, in that the interthecal wall, mentioned on the anti-sicula side, takes its origin at first nearer the middle of the left theca. From this it follows that, simultaneously with the two above-mentioned canals, the left theca is also developed from the bud on the sicula, but that the development of the same proceeded from the sicula side, where the connection exists.

The interthecal wall between the left theca and succeeding theca —the proximal part of the common canal is therefore formed falsely (*skeft*) as well as obliquely—ends on the anti-sicula side, and opens towards the common canal on the sicula side. The left theca, in consequence of this, as shown by its outlines (Figs. 2 and 3), has



A restoration of *Hyænodon cruentus*, Leidy. One-seventh natural size, from the White River (Oligocene) Beds of South Dakota.

a narrow and sack-shaped termination on the anti-sicula side. From all this it follows that the left branch near its proximal part is not "bilaterally symmetrical," in opposition to what Moberg believed was the case in the genus *Didymograptus* (Nyagrapt. från Skåne, p. 346). The development of the following thecæ on the left branch seems to be regular. The "connecting" canal, from which, on the opposite side of the sicula (right side), the right theca and common canal of the right branch arise, crosses the sicula somewhat obliquely, so that the right branch comes to originate somewhat nearer the aperture of the sicula than does the left branch. The lines of growth on the connecting canal are quite continuous with those on the right branch. It appears from Fig. 3 that the connecting canal is really separate from the sicula. At different foci under the microscope the growth-lines of both can be seen crossing one another. In the broken and ragged part of the connecting canal the growth-lines of the sicula alone are shown. Also in the forms preserved in relief of the anti-sicula side (Fig. 2) the "connecting" canal appears, and its passage into the canal from the sicula and common canal of both branches is very conspicuous. The aperture of the sicula is always bent to the same quarter as the apertures of the thecæ of the right branch.

DIDYMOGRAPTUS GRACILIS, Törnq. Mut. Pl. XIII, Figs. 7, 8.

This specimen has not been found transparent, but the same position of relief as in *Didymograptus minutus*, Mut., is shown on both sides. Fig. 7, sicula side; Fig. 8, anti-sicula side. The "connecting" canal is here conspicuous; the proximal end of the sicula shows a fine initial apex.

(*To be continued in our next Number.*)

II.—A RESTORATION OF *HYÆNODON*.

By Prof. W. B. SCOTT, F.G.S.

(PLATE XIIA.)

HYÆNODON is one of the genera of fossil mammals which has been for a long time imperfectly known. Excellently preserved skulls have been described and figured by Leidy and Filhol, and some of the limb-bones and vertebræ by De Blainville and Schlosser. Material was, however, lacking for a satisfactory restoration of any of the species, and the specific reference of the scattered bones found has always remained more or less uncertain. In the season of 1894 Mr. Hatcher had the good fortune to discover in the White River beds (Oligocene) of South Dakota a number of remarkably fine specimens pertaining to several species of *Hyænodon*, which at length enable us to gain an idea of the appearance of this most remarkable animal.

The accompanying figure (Plate XIIA) must at once strike everyone as altogether grotesque and improbable, and were it not so largely drawn from the remains of a single individual found together in one block of matrix, I should not venture to publish it. To this individual

belong the skull, neck; nine thoracic, four lumbar, two sacral, and two caudal vertebræ; the scapula, ulna, radius, and part of the carpus; the femur, tibia, fibula, and part of the pes. The missing vertebræ were restored from a second specimen which had the skull, all the presacral vertebræ, tibia, fibula, and hind foot. This is a young animal, still retaining the milk dentition, and is very probably referable to the same species (*H. cruentus*, Leidy) as the first. Another adult specimen of the same species, slightly larger than the first, consists of the mandible, axis, humerus, ulna, pelvis, and femur, and allows the proper proportions to be accurately observed. Other more or less fragmentary skeletons, representing various species, lead to similar results, though it yet remains to be determined how far the European species, which exhibit certain constant differences from the American, were characterized by the same remarkable appearance.

As compared with any of the recent Carnivora, the head of *Hyænodon* seems large out of all proportion to the body and limbs, the neck short, the back, especially the lumbar region, quite long, the tail short, the limbs short and slender, and the feet weak.

The characteristic and peculiar physiognomy of the head is already familiar and needs no further description. The neck seems very short and slender to carry the weight of the large head, its length being hardly more than two-thirds that of the skull. The axis is the only cervical vertebra which is strongly developed, possessing a large spine; the others are weak.

The thorax is small, when compared with the skull, but measured by any other standard is quite large and capacious. The vertebral spines are developed much as in the Carnivora, and the transverse processes and rib-tubercles are very conspicuous. The lumbar region is quite long and powerful, the vertebræ having massive centra, long and heavy spines, transverse processes, etc. These features are most marked in *H. horridus*; the smaller species have much less massive loins and evidently feebler muscles. The whole back, from the neck to the sacrum, is strongly axled upward, and its parts are articulated together with unusual flexibility and strength. The tail is rather short and slender, and has about the same relative length as in the raccoons.

The scapula is small and, though with many peculiarities (especially the shape of the acromion), has a shape not unlike that found in the dogs. The humerus is short and slender, and the fore-arm bones still more so, though the ulna is relatively little reduced and has, as in nearly all creodonts, a very prominent olecranon. The manus is small, short, and broad, with five spreading digits and short phalanges, terminated by heavy claws.

The pelvis is decidedly large, even when compared with the overgrown skull, and has an expanded, flattened ilium, more carnivorous than creodont in appearance. The femur considerably exceeds the humerus in length, but is light and slender and has nearly lost the third trochanter. The tibia is short, though much longer than the radius, and the fibula is stout, with expanded ends.

The pes is pentadactyl, and the gait was probably semiplantigrade, like that of many of the recent viverrines. The remarkable peculiarities of *Hyænodon* may, as Filhol has suggested, perhaps find their explanation in the aquatic habits of the animal.

The structure of the European species of this genus is not nearly so completely known as is that of the American. In dentition and skull structure the two groups of species are nearly identical, and probably the characters of the rest of the skeleton corresponded as closely. In two points, however, the European forms, so far as they are known, deviate from the American: (1) in the presence of an alisphenoid canal, and (2) in the formation of a scapho-lunar bone in the carpus by the coalescence of the scaphoid, lunar, and central, which in the American species remain permanently separate.

This is hardly the place to enter upon a discussion of the relationships and phylogenetic descent of *Hyænodon*. It may be mentioned, however, that the problem is much modified by the discovery of *Hyænodon* in the American Upper Eocene (Uinta beds), recently announced by Osborn and Wortman.

Geological Museum of Princeton University.
August 12th, 1895.

III.—Review of the Evidence for the Animal Nature of Eozoön Canadense.

By Sir William Dawson, K.C.M.G., LL.D., F.R.S., etc.

I. Historical and Stratigraphical.

THE writer of these notes had hoped to have been able long ago to let the vexed questions respecting Eozoön repose in peace in so far as he was concerned, and he is now induced to offer a short summary of the evidence in the case only with the view of correcting some misapprehensions that seem to have arisen in regard to points well established, and which, independently of any question as to the nature of Eozoön, belong to the certain data of geology. These misapprehensions lead to the confounding of the structures originally discovered by Logan with things in no way related to them, and from which they had been clearly distinguished by my own original studies, and by those of Hunt, Carpenter, and Rupert Jones. New facts relating to pre-Cambrian life have also been coming to light from time to time, and many of these are connected, either directly or indirectly, with the evidence respecting Eozoön.

As early as 1858, Sir William Logan had begun to suspect that the Stromatoporoid forms collected from the great Laurentian limestones in different parts of Canada must be of organic origin, and he ventured to mention them as possibly of this nature at the meeting of the American Association in 1859, and in his General Report on the Geology of Canada in 1863. The evidence on which he relied was their occurrence only in the limestones, their similarity in form and general structure to the Stromatoporæ, or "Layer-Corals" of the Palæozoic, and the circumstance that, while the forms and structures seemed to be identical, they were mineralized

by Serpentine, Loganite, Pyroxene, and Dolomite, an indication that a similar mould had been filled by diverse minerals.

At that time the little leisure I could spare for original work was occupied with Carboniferous and Pleistocene geology, and I had no ambition to invade the great and difficult pre-Cambrian districts of Northern Canada any farther than might be necessary to my work as a teacher of geology. In the interest of that work, however, I had gone over considerable portions of the Laurentian and Huronian districts surveyed by Logan and Murray, with the aid of their maps and reports, and had satisfied myself of the great accuracy of their work, which led in my judgment to the following results:—

(1) That the upper part of the Lower Laurentian of Logan, since called the Grenville Series,[1] consisted of truly stratified metamorphic rocks, including great and extensive deposits of limestone, quartzite, iron-ore, and other rocks, evidently of aqueous origin, and that the condition and crystalline and chemical characters of these rocks were not essentially different from those of the altered Palæozoic beds with which I was familiar in Nova Scotia and New England.

(2) That the Huronian, a less disturbed, less altered, and in the main evidently a clastic series, rested unconformably on the Laurentian, and was in part composed of its materials.

(3) That the "Upper Copper-bearing series" of Lake Superior, since known as Kewenian, was newer than the Huronian, but older than the oldest fossiliferous Cambrian rocks then known in Canada.

(4) That, while the Kewenian and Huronian rocks, and those designated by Logan as Upper Laurentian, indicated by the presence of igneous masses, and, in the case of the two former, by the prevalence of coarse, clastic material, littoral conditions and much volcanic disturbance, the still older Grenville Series was of a character more indicative of long-continued quiescence, accompanied by the accumulation of great calcareous deposits, possibly of organic origin.

These conclusions were noticed in papers contributed to local societies, in published lecture-notes, and in class-teaching, and were frequently discussed with Logan and Hunt. Accordingly, when, in 1863, at the urgent request of Logan, I undertook the microscopic examination of large series of his supposed Laurentian fossils and the containing limestones, as well as of other crystalline limestones of various ages, slices of which he had caused to be made, I was not unprepared to find the curious and beautiful structures which developed themselves in his Stromatoporoid forms, and in portions of the limestone in which they were contained, but which appeared to resemble those of Foraminifera rather than those of Corals.

The results thus attained, in 1864, were not fully published until after Logan was prepared to sustain them by detailed maps and sections of the district on the Ottawa containing Eozoön, a work extending over many years of arduous and skilful labour; and until Dr. W. B. Carpenter and Prof. Rupert Jones had studied the original specimens and others prepared for themselves, along with

[1] By Dr. Sterry Hunt.

my notes, and camera drawings prepared by the artist of the Geological Survey. Dr. Sterry Hunt had also examined chemically the serpentine and other minerals associated with the supposed fossils, and various hydrous silicates mineralizing organic remains in Silurian and other limestones, as terms of comparison. The whole was then communicated to the Geological Society of London, and appeared in the somewhat elaborate joint paper published in 1865.

[But a preliminary account entitled "On the occurrence of Organic Remains in the Laurentian Rocks of Canada," by Sir W. E. Logan, F.R.S., F.G.S., with communications on the structure by J. W. Dawson, LL.D., F.R.S., and on the Mineralogy of the same remains by T. Sterry Hunt, F.R.S., had, however, been communicated to the British Association at Bath, Sept. 15–21, 1864, and was subsequently published in the GEOLOGICAL MAGAZINE, Vol. I, for November 1864, pp. 225–227.]

I confess that in the intervening time I have seen no good reason to induce me to doubt the essential validity of the work embodied in this paper of 1865, or to modify to any considerable extent the conclusions therein stated. On the other hand, many new and confirmatory facts have been disclosed, and after careful and, I trust, candid study of the objections raised, down to those which have recently appeared in the Dublin Transactions, I believe that they largely depend on want of knowledge of the character of the Grenville formation, and on misapprehension as to the form and structure of Eozoön and its mode of occurrence.

It is true that in those members of the Laurentian system of Logan which are below and above the Grenville Series, later observations have not only failed to detect fossils, but have shown valid reasons adverse to the probability of their occurrence, at least in the portions of those formations hitherto open to our study.[1]

The lowest Laurentian gneiss of Logan (Trembling Mountain gneiss, Ottawa gneiss, fundamental gneiss), which occupies a vast area in Northern Canada,[2] and is the only part of the system known to many geologists, consists, so far as known, wholly of foliated or massive orthoclase gneiss, with bands of hornblendic schist (amphibolite), and of hornblendo-micaceous schist. While in some places it appears to have a truly bedded structure, especially where different varieties of gneiss, amphibolite, and biotitic schist alternate, in others its foliation is obscure, or seems to have been induced by heat and pressure. Dr. F. D. Adams, who has given much study both to its characters on the large scale, and to the microscopic structure of the rocks, in his latest publication on the subject[3] characterizes it as a complicated series of rocks of unknown origin,

[1] See GEOLOGICAL MAGAZINE, June, 1895.

[2] According to the geological map of Northern Canada prepared by Dr. G. M. Dawson for the Geological Survey, the area of Laurentian rocks exceeds two millions of square miles. Of this, so far as is known, the older or fundamental gneiss occupies by far the larger portion.

[3] American Journal of Geology, vol. i, No. 4, 1893.

but comprising a considerable amount of intrusive material. He regards it as either the remains of a primitive crust penetrated by much igneous matter, or as a series of altered rocks older than the Grenville Series, and formed under different conditions. In any case it seems to want the evidences of ordinary aqueous deposition presented by the limestones, ironstones, quartzites, and schists of the Grenville Series. Similar views were advocated in my address on the "Geological History of the Atlantic," before the British Association, in 1886.[1]

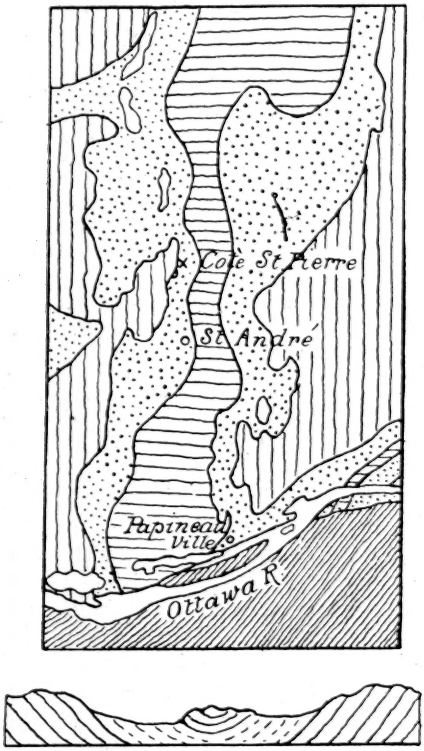


Fig. 1.—Distribution of Grenville Limestone in the district north of Papineauville, with section showing arrangement of the beds. Scale of map 7 miles to an inch. (See also Dr. Bonney's paper, Geol. Mag., July 1895, p. 295.)
Dotted area: Limestone.
Horizontal lines: Upper gneiss (fourth gneiss of Logan).
Vertical lines: Lower gneiss (third gneiss of Logan).
Diagonal lines: Overlying Cambrian and Cambro-Silurian (Ordovician).

The Upper Laurentian of Logan (Labradorite, Anorthosite, or Norian Series), supposed by him to overlie the Grenville Series unconformably, is now stated by Adams to consist of eruptive

[1] See also Museum Memoir on Eozoön, pp. 2, 3. Montreal, 1888.

matter, mainly composed of triclinic or lime felspars, and to which the name Anorthosite[1] may properly be applied. These rocks, cutting the Grenville Series, and apparently in some places interbedded with it, are not now regarded as a distinct series of beds, but as indicating local outbursts of igneous action dating about the close of the Grenville period. What aqueous rocks may have been contemporaneous with them, or may have filled the interval between the Grenville Series and the Huronian, we do not at present certainly know, though possibly some of the rocks associated with the upper part of the Laurentian, or the lower part of the Huronian in the interior, and in the eastern part of Canada, may come into this place.[2]

It is to be observed that in 1865 these facts respecting the fundamental gneiss and the Upper Laurentian of Logan, were not distinctly before our minds, though in subsequent papers I thought it best to consider the Grenville group as a distinct series under the name "Middle Laurentian." It is quite possible, however, that our referring in the first instance to the Laurentian as a whole, may have led to erroneous impressions.

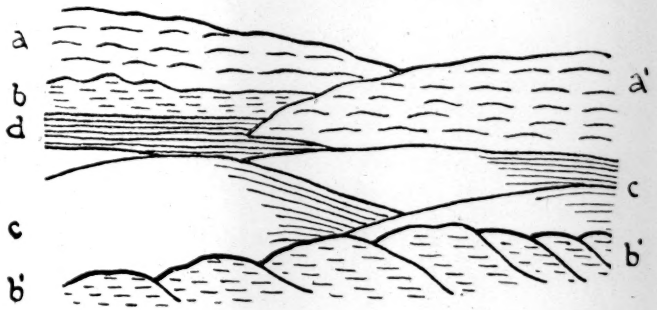


FIG. 2.—Topography at Côte St. Pierre, from the N.E. (For section see Dr. Bonney's paper, GEOL. MAG., July 1895, p. 296.)

(*a*) Lower gneiss. (*a'*) The same brought forward by fold.
(*b*) Limestone (Lorne's Quarry). (*b'*) The same, exposure on La Vigne's farm.
(*c*) Limestone, mostly covered with soil.
(*d*) Pond or small lake.

For the purpose of these notes, therefore, it will be best and most accurate to confine ourselves to the Grenville Series, which has been carefully explored and mapped by the officers of the Geological Survey in the country lying north of the Ottawa River, and also in some parts of the areas between that river and the St. Lawrence. In these regions Logan recognized a thickness of 17,250 feet of deposits, of which no less than 4,750 feet consisted of limestone, principally in three great bands, though with intercalated gneissose

[1] Proposed by Hunt.

[2] Some of these beds are regarded by Von Hise (Amer. Journ. of Geology, vol. i) as a lower member of the Huronian. They may also be identical in part with the "Kewatin" group of Lawson.

layers. The Grenville Series may thus be regarded as one of the great calcareous systems, comparable with those of the Palæozoic period, which it also rivals in its association with carbonaceous and ferruginous deposits. Though minute globular forms, probably

FIG. 3.—Arrangement of beds in valley of Calumet River—(*a*) Upper gneiss; (*b*) Limestone partly covered with soil; (*c*) Included bed of gneiss; (*d*) Lower gneiss.

organic, have been found in the Middle Limestone, that of Long Lake, Eozoön proper is confined, so far as known, to the Upper Limestone, known specially as the Grenville Band. This band and its accompaniments I have myself studied in the region north of

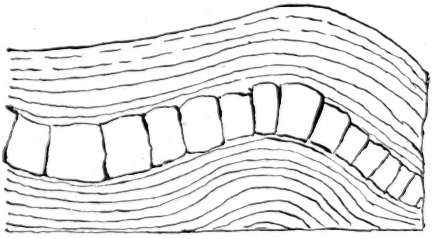


FIG. 4.

the Ottawa, at the Augmentation of Grenville, near the Calumet, in the quarries opposite Lachute, at Côte St. Pierre, at Montebello, at Buckingham, and Templeton, as well as in some of the districts west of the Ottawa, where the same limestone is supposed to recur.

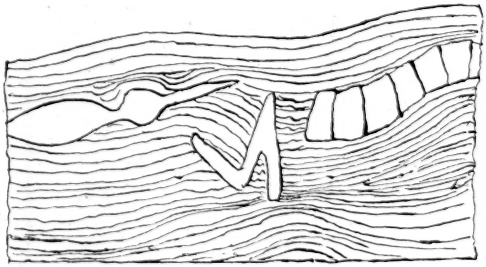


FIG. 5.

FIGS. 4 and 5.—Bent and dislocated Quartzite, in contorted schists interstratified with Grenville Limestone, near Montebello. The quartzites have been broken and displaced, while the schists have been bent and twisted. In the immediate vicinity the same beds may be seen slightly inclined and undisturbed.

Everywhere it is a large and regular bed, sometimes with even strike and dip, but at intervals thrown into violent contortions along with the enclosing beds, in the manner usually seen in disturbed strata of later age, where it is common to find portions little affected

by plication alternating with strongly folded beds having the harder ones dislocated; others are merely bent or folded (Figs. 4 and 5). It presents subordinate beds of different qualities, dolomitic, serpentinous, or graphitic, and is immediately associated with thin-bedded, fine-grained gneisses, quartzite, and biotitic and hornblendic schists. In some beds it has disseminated crystals of minerals usually found in metamorphic limestones, while in others there are concretionary masses, nodules, and grains of serpentine and pyroxene. Eozoön in masses occurs only in certain layers, most frequently in those which are serpentinous, but a careful examination detects in many layers, not showing perfect examples of Eozoön, small fragments or patches having its characteristic structures, or detached chamberlets or groups of these. The occurrence of these fragments I regard as an important fact, and as showing that what may be termed "Eozoön sand" enters largely into the composition of the limestone.

In illustration of this part of my subject, I present a rough map of the district near the Petite Nation River, in rear of Papineauville, referred to by Dr. Bonney in his valuable paper in the July Number of this MAGAZINE, and in addition to the section given in his paper, one showing the order of succession in the valley of the Calumet, a little stream some distance to the eastward. I also give examples of the manner in which the associated gneiss, though often very regular, is along certain lines contorted, and the manner in which, in these contorted spots, the quartzite bands are cracked and broken, exactly as may be observed in the shales and sandstones of the Quebec group on the Lower St. Lawrence.

I may add here that Dr. Adams has found that in certain localities the rocks of the Grenville Series become almost horizontal, though even in this case they show evidence of having been subjected to much alteration and to lateral pressure.

The summary of facts above given should, I think, be sufficient to show that in the case of the Grenville limestone we have phenomena which cannot be explained by mere pressure acting on massive rocks, or by segregation of calcite from igneous rocks, or by vein structures, or by any contact structures arising at the junction of igneous and aqueous deposits. We have, on the contrary, to deal with a formation which indicates that in the early period to which it belongs regular sedimentation was already in full operation. The more precise vital and chemical agencies which prevailed in the ocean of the Laurentian period we must notice later.

I have merely to add here that the characters assigned above to the Grenville Series have not only been fully corroborated by the recent work of Adams and Ells in Canada,[1] but also by the surveys of Kemp and Smyth in the more disturbed and elevated district of the Adirondack Mountains in New York.[2]

We have thus paved the way for the consideration of evidence of a structural and chemical character.

(*To be continued.*)

[1] American Journal of Geology, 1893, No. 4. Also Reports Geol. Surv. of Canada.
[2] Bulletin Geol. Soc. of America, March 1895.

IV.—The Glacial Deposits of Aberdeenshire.

By Prof. Edward Hull, M.A., LL.D., F.R.S.

MR. DUGALD BELL, F.G.S., has been so good as to send me a copy of his paper on the "Shelly Clays and Gravels of Aberdeenshire,"[1] on which I beg leave to offer a few observations, as it deals with a subject in which I take much interest, namely, the origin and mode of formation of such deposits. His paper is mainly based upon one by Mr. T. F. Jamieson, of Ellon, also published in the same journal,[2] and on a recent memoir by the Geological Survey of Scotland.[3] There is no difference amongst these authors of any importance as regards the structure and composition of these post-Tertiary deposits, which are found adjoining the coast of Aberdeenshire; and they may be briefly described as follows, in descending order[4]:—

1. Upper Red Clay with boulders, and a few marine forms, 1 to 15 ft.
2. Gravel and Sand with waterworn pebbles and shells, 40 to 60 ft.
3. Lower Boulder-clay ("Grey rubbish"), 5 to 10 ft.

As regards No. 3, there is no difference of opinion. All are agreed that it is the product of the great ice-sheet—the *moraine profonde*—of the epoch of intensest glaciation. It is when we come to deal with Nos. 2 and 1 in the above series, that we find divergence of opinion amongst the authorities quoted. Thus, Mr. Jamieson considers the gravel and sand (No. 2) to be a sort of "moraine," though in some way not clearly explained, "washed" or reconstructed under water, owing to which the pebbles have lost their glaciated surfaces; in fact, the pebbles are remarkable for the absence of glacial striæ, which one would have supposed was strongly indicative of aqueous action. The bedding is generally rude and irregular, but current-bedding occurs occasionally, and the marine shells are almost always fragmental. Notwithstanding all these indications of deposition under water, Mr. Jamieson does not consider that the beds indicate submergence. Mr. Bell concurs, and regards the No. 2 series as the product of land glaciation only (p. 475). On the other hand, the writer of the Survey Memoir considers this deposit as "evidence of inter-Glacial submergence."

I may here pause to enquire in the first place why, if No. 2 deposit has an identical, or similar, origin with No. 3 (the Lower Boulder-clay), there should be so marked a distinction between them? It certainly requires some very clear demonstration to induce one to believe that one ice-sheet will give rise to a stiff unstratified clay with glaciated stones and boulders; and another, to a rudely stratified gravel and sand in which the pebbles are subangular or rounded and destitute of glacial striæ. Even Mr. Jamieson sees the

[1] Quart. Journ. Geol. Soc., Aug. 1895.

[2] *Ibid.*, vol. xxxviii.

[3] This I regret to say I have not seen; but rely upon Mr. Bell's quotations therefrom, which I doubt not are correct.

[4] Very clearly represented in Mr. Jamieson's Coast-section, *ibid.* p. 151.

difficulty, and suggests the washing and rounding process above referred to; but if this were carried on under the waters of the sea it involves the recognition of submergence, a view borne out by the presence of sea-shells.

Next we have to consider the mode of formation of the Upper Red Clay (No. 1) which is found overlying the gravel and sand deposit (No. 2). Mr. Jamieson considers this to be undoubtedly the product of submergence to a depth of some 300 feet; but Mr. Bell regards it as of purely glacial (or land-ice) origin. It is not surprising that as Mr. Bell regards No. 2 as the product of land glaciation, he should take a similar view with regard to the Upper Red Clay with boulders. But again I ask—why, if all these three deposits are the products of similar physical agencies, should they assume such diversified forms as to be capable of, not only definition under different terms, but of illustration along the line of a sea-cliff under very different modes of representation, as Mr. Jamieson has shown? If they have originated from one and the same agency, why should they not have been found as represented by a solid mass of Boulder-clay from top to bottom? The same or similar conditions would, we may suppose, have produced similar results. Instead of three deposits superimposed and each differing from the other, we might have expected one solid ground-moraine formation throughout. Then, in order to maintain the purely land-glacial origin of No. 1, according to Mr. Bell, we have to explain in some way (to myself unintelligible) the presence of marine forms, enumerated by Mr. Jamieson,[1] consisting of shells, such as *Astarte borealis*, *Cyprina islandica*, *Saxicava sulcata*, etc.; remains of fishes, swimming birds (eider-duck), seals, starfishes (*Ophiura*). These forms were not found, it should be observed, on or near the surface, but sometimes imbedded deep in the clayey mass. No doubt Mr. Bell would reply that their presence is to be accounted for by supposing that these forms were carried from the sea to the land by the glacier.[2] But if this be the explanation, I would observe that the ice of the North Sea, whether derived from Scandinavia or Scotland itself, was only forced over the land while the North Sea was blocked by ice, in which there were neither shells, starfishes, nor probably seals. On the whole it would seem that Mr. Jamieson has on this point the preponderance of the evidence; and if so, considering the sub-glacial conditions under which the Upper Boulder-clay was clearly deposited, it does not seem surprising that in the waters "marine life was far from abundant."

It seems to me that the confusion which has arisen amongst the neo-glacialists regarding the glacial phenomena of the British Isles is largely due to the practice of confounding aqueous with glacial deposits. As long as deposits, such as No. 2 above, consisting of

[1] *Supra cit.*, p. 166.

[2] If it be the intention of Mr. Bell to represent this by his map, I may observe that on comparing the lines of the ice movement with the arrows given by Prof. James Geikie in his map in the "Great Ice Age," they are almost always at right angles: both cannot be correct.

beds of sand, gravel, and laminated loam or clay, are looked upon as the production of land-ice, so long will these enthusiastic glacialists fail to arrive at intelligible conclusions regarding the physical conditions of the Glacial period. Is it not much more rational to suppose that three superimposed deposits, differing in composition and structure from each other, should have been formed under three different sets of conditions rather than under one and the same or similar? And in the case of the Aberdeenshire deposits surely we have a very evident succession of conditions, such as, in brief: 1st, general land glaciation; 2nd, submergence in sea-waters, during which the rivers brought down sand and gravel from the adjoining emergent lands to be taken up and distributed over the sea-bed; and 3rd, continued submergence, with the recurrence of cold conditions, owing to which the snows and glaciers again occupied the higher elevations, and the streams charged with glacier mud entered the sea and gave rise to a Red Clay formation. The Aberdeenshire deposits do not stand alone; they have their equivalents in Lancashire and Cheshire and in Ireland. The fine section on the banks of the Ribble above Preston is identical (*mutatis mutandis*) with that on the coast of Scotland, and indicates similar widespread conditions in the Glacial period. Mr. Bell denies submergence; but I am tempted to quote a passage from a letter recently received from Prof. Prestwich on this subject, which may possibly have some weight with the neo-glacialists.[1] He says: "I quite agree with you as to the important submergence in Glacial times which has left such clear evidence in the Cotteswold Hills[2] and Welsh Mountains. It is surprising to me that the ice-ploughing hypothesis could ever be entertained. Some half century ago it was my good fortune to come across some fossiliferous gravels in the hills (1,120 feet) between Chesterfield and Buxton. Why the simple explanation should ever have been pushed aside in favour of the more fanciful view I cannot understand, unless it be the innate love of change." Neither can the writer.

V.—On the Pitch Lake of Trinidad.

By S. F. Peckham.

(*Continued from the September Number*, p. 425.)

NEAR the crest of the ascent to the lake the road divides: one branch passing to the left and south ascends over the rim of the basin of the lake, and skirting the lake for about a quarter of its circumference passes over the hill to the south-west, as described by Mr. Manross. The right-hand branch follows the flow of the pitch and enters upon the lake simply by a change of grade from a sharp ascent to a very slight inclination upwards towards the centre of the lake. I was particularly impressed with this fact, and took pains

[1] Dated 25th July, 1895.

[2] On the Jurassic table-land of the Cotteswolds we find round quartzite pebbles, washed out of the New Red Conglomerate of the Midlands, scattered over the surface up to a level of about 600 feet—a real "Northern drift."

to verify my first impression upon a second visit, as it proved conclusively that, notwithstanding the vast quantities of pitch that had been removed from the lake, there is still a movement out of the lake, glacier-like, down the slope to the sea.

My first impression as I looked over the expanse of the lake was a surprise. I had expected a scene of desolation. Nothing could be further from the reality. In the centre were the islets so often described. Within and around them a dark area resembled the muddy bottom of a pond from which the water had been drawn off, with here and there patches and intervening streams of water remaining. From the border of this dark centre, the vegetation arose higher and higher around almost the entire circumference of the lake, until it reached a border of palm trees from thirty to fifty feet high. As I looked over the lake I beheld on a vast scale the appearance of asphalt beds that I had many times seen in California.

An examination of the borders of the lake showed that it occupied a bowl-like depression in a truncated cone that rested against the side of a hill that rises above the lake to the south-west. Along the line of ascent that I had followed, the slope towards the north-east to the sea is very gradual. In other directions the ascent is abrupt, sometimes steep, especially toward the south. These slopes are covered with tropical jungles consisting of palms of various species, sedges, canna, and wild vines. The border of this depression presents upon the inside for the most part an escarpment of sand and clay, that has evidently been built up and afterwards broken down in many places by water. Wherever excavations have been made in the cone or the escarpment they show that the cone consists of both asphalt and earth. At a point on the south side, near where the road leaves the lake, the appearance of the surface indicates that the drainage of water from the lake was frequently in that direction to a considerable amount, notwithstanding numerous artificial drains lead out of the circumference of the lake and the wide natural outlet down the slope to the sea. To the north-west towards the sea, a heavy stream of asphalt has overflowed to the sea, forming a barrier reef for a considerable distance. Asphalt has also overflowed to the south, and the general appearance of the escarpment seemed to indicate that at some remote period the basin now occupied by the lake had been filled some three feet higher than the present level of the lake. I looked in vain for any evidence that the mass within the lake had been recently depleted; but I am aware that observations at considerable intervals of time would be necessary to establish that fact, by referring the mean level of the lake to some fixed point by means of a very careful trigonometrical survey.

A very careful study of the present appearance of the lake and its boundaries led me to believe that the suggestion of Mr. Richardson, that the lake occupies the crater of an old mud volcano, is correct, and that it has been built up of very unstable material, through contact of water issuing in large quantity from subterranean springs which has come in contact with strata identical

with or resembling those described by Mr. Guppy.[1] Into this ascending current, resembling a quicksand, was projected bitumen, at intervals in very large amounts, so that irruptions of mud have coincided with and alternated with irruptions of bitumen, the whole building up the cone and at times overflowing it, while the basin has gradually filled with bitumen to the exclusion of mud.

It is, however, equally evident that for an indefinite period there has been an outflow of bitumen from the crater towards the sea, at La Brea, not over its rim, but through a crevice in its side—in fact, through its broken-down side; and that, notwithstanding the vast quantities of asphalt now being taken from the lake by the Concessionaires, the movement is still out of the lake.

Capt. Alexander, in 1832, spoke of the flow out of the lake as "immense." Manross, in 1855, says, "this stream of pitch has been dug through in several places, averaging from 15 to 18 feet in depth." A well dug at one point on the slope of the overflow was abandoned still in asphalt, at the depth of forty feet. Several village lots have been excavated to a depth of twenty feet, still in asphalt. The invariable reply of the negroes to the question, "Have you ever dug through the asphalt?" was, "No, sir." The conclusion that I reached on the ground is, that the asphalt flowing down the slope to the sea fills a ravine excavated by water, and that it is slowly moving out of the lake with the pressure of the asphalt in the lake behind it. This conclusion is in harmony with the testimony of all the observers above quoted for the last hundred years.

Concerning the condition and appearance of the pitch within the lake, I think it is quite certain from all the observations above quoted that the pitch has gradually become harder and more stable during the last 106 years. I do not think that later observers have any right to question the veracity of those who have preceded them. Dr. Nugent says that in 1807 the centre was so soft that it could be dipped up with a cup. Alexander describes it in 1832 as so unstable that the weight of a man produced a bowl-like depression to the depth of one's shoulders, and that the heat gradually increases as one walks off towards the middle with his shoes off. Manross, 23 years later, says, "It may be that the material has become much harder since the first accounts of it were written; but it is difficult to understand how the weight of a man can have displaced a mass of pitch equal to a 'great bowl' as deep as the shoulders." Kingsley, 24 years later, is practically of the same opinion. At the time of my visit, a man was loading a cart near the centre of the lake, and while they did not remain in one place long enough to secure a large load, there was no apparent danger of their being engulfed.

[1] Guppy says (Quart. Journ. Geol. Soc., xlviii, 527, note): "When a piece of the foraminiferal rock is placed in water, it absorbs it rapidly and falls asunder, and the water which enters into union with it is given up only to evaporation. . . . From these properties it follows that the natural soil roads passing over these rocks become in the wet season the worst quagmires it is possible to imagine." Of another bed, "In the presence of water this rock is the most incoherent of any I have ever met with. . . . It falls into powder at the mere contact of water."

Yet it must not be assumed that the cosmical agencies that produced this deposit of bitumen have ceased to be active, or are even simply quiescent. Abundant evidence is to be found in the neighbourhood, and even within the lake itself, that such forces are still active. A few miles to the south-west of the lake, at Guapo, large springs of maltha or "liquid asphaltum" are now flowing, and within the boundaries of the lake near the power station of the tramway, and within only a few rods of the edge of the cone, I observed what the workmen called (and very properly) a "blow-hole." This was a circular hole, about six inches in diameter, from which bitumen, more nearly fluid than any I saw elsewhere upon the island, had been ejected to the amount of perhaps a barrel. It was so soft as to flow readily, of a brilliant black colour, and appeared to contain little, if any, mineral matter. I was told by a workman that such holes occurred quite frequently and so far apart as apparently to have no connection with each other.

Asphalt beds occur in California that are the product of the hardening of maltha, or mineral tar, which escapes over a considerable area. Sometimes it flows continuously from a central orifice, but oftener the flow through the hot summer seems to be arrested by the lower, winter temperature, when the orifice through which the flow took place becomes plugged. The succeeding season the maltha issues along a line of less resistance and flows through the summer, when it in turn becomes plugged. These plugged orifices are often several rods apart for successive seasons, and present the appearance of a cicatrix. I have no doubt that more extended observations than I was able to give would reveal a similar condition of outflow at and in the vicinity of the lake.

I carefully studied these phenomena as likely to offer some suggestion concerning the origin of the deposit. As a description of the observed facts, I can add nothing to that of Mr. Manross. I do not understand why Messrs. Wall and Sawkins observed nothing of the sort described by him, or thought it not "very obvious to what force or what influence this is attributable." These "areola" are very irregular in shape. I think their form may be, to some extent, determined by the weight of water pressing against their sides. The surface of each one is slightly rounded from the centre to the edge of the water; they then round off at a very sharp angle, finally descending almost perpendicularly. These areas consist of pitch inflated with gas to such an extent, that when broken into the structure exactly resembles an over-fermented cheese—hence the term "cheese-pitch." The cavities are from one to three or more inches in dimensions. The gas that they contain is constantly rising to the surface, where it bubbles out and bursts, thus forcing the centre up, and causing a slow but irresistible movement from the centre towards the circumference, where the pitch continually rolls under exactly as Manross has described it. His suggestions concerning the ebullition of the mass within the lake were confirmed to the very letter.

This action is explained in this wise: Mr. Richardson's analysis

of asphalt water shows it to be very rich in sulphates. As has been elsewhere shown, sulphates, especially those of the alkalies, when in solution are decomposed, when the water containing them flows through strata impregnated with organic matter, into hydrogen sulphide, and a carbonate of the oxide present. When hydrogen sulphide infiltrates strata containing carbonate of lime, gypsum is formed and sulphur deposited, or converted into free sulphuric acid.[1]

The Miocene bituminous strata of Southern California are full of sulphur springs and numberless deposits of sulphur. One such deposit in the southern part of Kern County is supposed to contain several thousand tons of sulphur.

The reaction between sulphates present in the lake water and the bitumen or other organic material of the formation furnishes a ready explanation of the presence of hydrogen sulphide; but I must confess that the odour of that gas was much less apparent about the lake than I had been led to expect. Analysis will alone show what the gases are that inflate the asphalt, but of their presence in enormous volume there can be no question. At a rough estimate, I should say that from one-third to one-half the volume of the mass as it exists in the centre of the lake is gas. I also hazard the opinion that this gas makes the *mass* specifically lighter than water, else the tables described by Manross and Kingsley would not rise and spread on the surface of the water, and further the masses of asphalt would coalesce, and the water would float upon the asphalt. Moreover, it is without any doubt that through this motion or ebullition which is produced, not by escape of vapour generated by heat, but by gases forced upward by their own specific gravity through a yielding mass, that the asphalt and mineral matter which forms the floors and sides of the crater are mixed together until the asphalt is saturated; *i.e.* it reaches such a condition of plasticity and viscosity, that it will no longer absorb any more mineral matter in presence of water. I cannot account for the almost uniform character of the mixture of water, bitumen, and mineral matter on any other hypothesis.[2]

Asphalt is very inert to changes of temperature. It is a very poor conductor of heat, and even under a tropical sun the daily surface changes of temperature and consequent expansions and contractions are wholly inadequate to produce conditions affecting such enormous masses of material as the crater contains.

The frequent use of the term "volcanic" in connection with the supposed origin of this mass of bitumen is, in my judgment,

[1] Proc. Amer. Philos. Soc., vol. x, p. 445. Bischoff, Chem. and Phys. Geol. (Cav. Soc. Ed.), ii, p. 28; *ibid.*, vol. i, pp. 15, 340. T. S. Hunt, Chem. and Geol. Essays, pp. 23, 87, 99, 111.

[2] Mr. Richardson asserts that 90 per cent. of the 80 per cent. of insoluble mineral matter in the pitch is silica. As a possible explanation of the presence of so much silica, I would suggest that the hot water that distilled the bitumen might have held silica in *solution*, which has been precipitated within the pitch as it has cooled. The fact, if it be a fact, that so much silica exists in the pitch as hydrate, may account for the large amount of water held in the pitch.

misleading. With the term volcanic is usually associated streams of melted lava, scoria, and pumice. The masses of porcellanite and jasper mentioned by all observers as found in the neighbourhood of the lake, do not require for their origin any "subterranean fires." It only requires that hot water, holding silica in solution under high pressure, shall percolate a bed of clay. The distillation of beds of lignite requires nothing more. In one case the product is red or yellow jasper, in the other a deposit of bitumen. The less the pressure the more dense will be the bitumen. Water will inevitably bring the bitumen to the surface, unless it is held down by impervious strata. If the water, accompanied by bitumen, encountered in its upward passage such strata as have been described by Mr. Guppy, a mud volcano yielding bitumen would be the inevitable result. It appears to me that all of these conditions are present in and about the pitch lake. They are exactly the conditions that have produced enormous tar springs and asphalt beds in California, excepting that there the strata necessary to produce mud volcanoes are wanting, but the porcellanites, the hot springs, the sulphur springs, and the bitumen, are all there, and in some localities on a scale that vies with Trinidad.

I looked in vain for specimens of wood in process of transformation into asphalt. I enquired of many intelligent men, and others connected with mining the pitch, if they had ever seen such specimens; they invariably answered "no." Two or three remarked that the wood never decayed in the pitch, that it came out as it went in. One man replied that "if it went in rotten it came out rotten." I saw in several excavations along the tramway masses of vegetable matter that appeared to have been converted into humus, and was told by the workmen that in time these masses would become incorporated with the pitch. Such masses account for the organic matter in solution in the lake water, and also for the amorphous organic matter not bitumen, observed by Mr. Richardson.

The Concessionaires of the lake have recently put in operation a tramway and pier by which the pitch can be very rapidly and easily removed from the lake to vessels lying at the pier. The tramway forms a loop, which in a general way may be said to pass just outside the circle of islets. (See map.) In building the tramway much of the vegetation on these islets has been destroyed. The laying of the tramway presented some peculiar engineering difficulties, that have been fully overcome. The islands float on the pitch, and I believe that they represent portions of the edge of the crater broken off during violent irruptions, and placed in and maintained in their relative positions through their relations to the various centres of ebullition into which the surface of the lake is divided. These islets, which largely consist of vegetable matter, float, while logs of wood and palm-tree ties sink in the pitch; it therefore occurred to Mr. Freeman, the engineer in charge of the work, to support his tramway on palm leaves, of which many specimens are twenty-five feet in length. This expedient has proved a complete success, not only upon the summits of the "areola," but in crossing the crevices that separate them. The tramway furnishes

a succession of admirable points from which to view the lake, as no difficulty is experienced in walking upon the ties around the entire loop. The cars are run in groups of four, which, when loaded, have a gross weight of about six thousand pounds. I carefully watched the passage of successive groups of these cars and could not observe any change of level in the road bed as they passed along; yet I am quite certain if a group had been allowed to stand for several hours, that both tramway and cars would have sunk in the pitch.

The pitch is excavated along this tramway upon the summits of the "areola." Wherever the surface of the pitch is broken, the vesicles are uniformly smaller as the pitch is taken from points removed from the centre of the lake. As the water dries out the vesicles collapse, and the colour changes from brown to bluish-black. If left long enough in the sun, any of the pitch, no matter from what spot it may be taken, will first melt upon the surface, and finally flow into a more or less compact mass. The pitch being dug by the Trinidad Asphalt Company, both within and without the lake, was brown when freshly dug, changing to black on exposure. The same might be said of that dug farther down the slope from village lots by the Trinidad Bituminous Asphalt Company. It was quite evident that as the pitch was taken from points farther and farther from the centre of the lake it had been subjected to more and more pressure, the gas being forced out as a consequence, the vesicles made smaller, and the specific gravity thereby increased. There are enormous masses of pitch within the lake that could not, in my opinion, be distinguished by the eye from the pitch taken from the village lots by either of the companies before mentioned. I am therefore quite at a loss to determine why Mr. Richardson alleges such a specific distinction between what he pleased to term "lake" and "land" asphalt. It appears to me to be a distinction without a difference.

For further facts concerning the commercial and economic relations of Trinidad Asphalt, the reader is referred to the report of Consul Pierce, which I believe to be one of the most complete and impartial of all the valuable consular reports issued by the State Department.

It was my intention to include in this paper some statistics regarding the enormous amount of asphaltum of different varieties shipped from La Brea since January 1st, 1890. When a friend applied to the custom house in Port of Spain for an official statement, he reported that such information had been refused, on the ground that such a statement would make public private interests, inasmuch as the Trinidad Asphalt Company had shipped several cargoes of "land pitch" to the United States since that date.

By referring to the maps the reader can clearly distinguish the relative positions of the lake and the adjacent portion of the island.

University of Michigan, Ann Arbour, Michigan,
April 15th, 1895.

NOTICES OF MEMOIRS.

I.—British Association for the Advancement of Science. Sixty-fifth Annual Meeting, held at Ipswich, Sept. 11–18, 1895.

List of Papers read in Section (C), Geology.

William Whitaker, B.A., F.R.S., F.G.S., President.

The President's Address. (See p. 461.)

F. W. Harmer.—The Southern Character of the Molluscan Fauna of the Coralline Crag tested by an Analysis of its Abundant and Characteristic Shells.

F. W. Harmer.—The Derivative Shells of the Red Crag.

H. W. Burrows.—Notes on the Stratigraphy of the Crag, with especial reference to the Distribution of the Foraminifera.

H. B. Woodward.—Note on a Section at the North Cliff, Southwold.

John Spiller.—Recent Coast-Erosion at Southwold and Covehithe.

Rev. E. Hill.—Observations on East Anglian Boulder-clay.

Rev. E. Hill.—Indications of Ice-raft Action through Glacial Times.

Rev. E. Hill.—On Traces of an Ancient Watercourse in West Suffolk.

Clement Reid and *H. N. Ridley.*—Further Notes on the Arctic and Palæolithic Deposits at Hoxne.

W. Whitaker.—Some Suffolk Well-sections.

Prof. W. J. Sollas.—Pitch Glaciers or Poissiers, and Illustrations of Glacier Movements.

Clement Reid.—Notes on the Cromer Excursion.

Prof. W. B. Scott.—On the Tertiary Lacustrine Formations of North America.

R. B. White—The Glacial Age in Tropical America.

Beeby Thompson.—Pre-Glacial Valleys in Northamptonshire.

W. W. Watts.—Notes on some Snowdonian Tarns.

Dugald Bell.—Report of the Committee on the High-level Shell-bearing Deposits of Clava, etc.

Rev. E. Jones.—Report of the Committee on the Calf-Hole Cave.

B. Harrison.—Report of the Committee on the High-level Flint-drift near Ightham.

C. E. de Rance.—Report of the Committee on the Rate of Erosion of Sea Coasts.

C. E. de Rance.—Report of the Committee on Underground Waters.

J. Lomas and *P. F. Kendall.*—Observations on Modern Glacial Striæ.

T. V. Holmes.—Notes on the Ancient Physiography of South Essex.

Prof. O. C. Marsh.—Restorations of some European Dinosaurs, with suggestions as to their place among the Reptilia.

J. Parker.—Report of the Committee on *Cetiosaurus*.

Prof. E. W. Claypole.—On the Cladodonts of the Upper Devonian of Ohio. (See p. 473.)

Prof. E. W. Claypole.—On the Great Devonian Placoderms of Ohio. (See p. 473.)

Montagu Browne.—Preliminary Notice of an Exposure of Rhætic Beds near East Leake, Notts.

G. F. Dollfus.—Probable Extension of the Seas during Upper Tertiary Times in Western Europe. (See p. 474.)

E. Van den Broeck.—On the present state of our knowledge of the Upper Tertiary Strata of Belgium.

Marcellin Boule.—Discovery of Fossil Elephant Remains at Tilloux (Charente).

Prof. J. Milne.—Earth Movements observed in Japan.

Prof. J. Milne.—Report of the Committee on the Volcanic and Seismological Phenomena of Japan.

Dr. H. J. Johnston-Lavis.—Report of the Committee on the Volcanic Phenomena of Vesuvius.

C. Davison.—Report of the Committee on Earth Tremors.

Prof. W. J. Sollas.—Report of the Committee on the Investigation of a Coral Reef.

O. W. Jeffs.—Report of the Committee on Geological Photographs.

Dr. F. H. Hatch.—The Auriferous Conglomerates of the Witwatersrand, Transvaal.

E. A. Walford.—Report of the Committee on the "Stonesfield Slate."

E. A. Walford.—Note on the Strata of the Shaft sunk at Stonesfield, Oxon, in 1895.

W. Whitaker.—The Trial-boring at Stutton.

Joseph Francis.—The Dip of the Underground Palæozoic Rocks at Ware and at Cheshunt.

F. W. Harmer.—The Importance of Extending the Work of the Geological Survey of Great Britain to the Investigation of the Deep-seated Rocks, by means of Boring. (See p. 476.)

Prof. H. A. Nicholson and *J. E. Marr.*—Notes on the Phylogeny of the Graptolites.

E. J. Garwood and *J. E. Marr.*—Zonal Divisions of the Carboniferous System. (See p. 474.)

Prof. T. R. Jones.—Report of the Committee on Palæozoic Phyllopoda.

M. Laurie.—Report of the Committee on the Eurypterid-bearing Deposits of the Pentland Hills.

Dr. H. Woodward.—On some Decapod Crustaceans from the Cretaceous Formation of Vancouver Island.

A. Smith Woodward.—Report of the Committee on the Registration of Type Specimens.

P. F. Kendall.—Report of the Committee on Erratic Blocks.

TITLES OF PAPERS BEARING ON GEOLOGY READ IN OTHER SECTIONS.

Report on Cosmic Dust.

Report on Underground Temperature.

A. Gobert.—The Gobert Freezing Process for Shaft-sinking and Tunnelling under Rivers.

J. Vivian.—East Anglian Coal Exploration, description of machinery.

W. J. Knowles.—On striated Flint Implements from North Ireland.

B. Harrison.—Report on Plateau Flints of North Kent.

H. Stopes.—Graving Tools from Terrace Gravels of Thames Valley.

H. Stopes.—Palæolithic Projectiles.

Graf Solms-Laubach.—On a new form of Fructification in *Sphenophyllum.*

C. W. Andrews.—On Stereornithes. (See p. 472.)

Dr. D. H. Scott, F.R.S.—Chief results of Williamson's work on the Carboniferous Plants.

A. C. Seward.—The Wealden Flora of England.

Wm. Barlow.—On the Relation between the Morphological Symmetry and the Optical Symmetry of Crystals.

Dr. F. H. Hatch.—Gold Production in the Witwatersrand Fields.

Dr. Conwentz.—On English Amber, with Exhibition of Specimens.

Dr. J. G. Garson.—A Palæolithic Skeleton from the Thames Valley.

II.—British Association for the Advancement of Science, Ipswich, 1895.—Underground in Suffolk and its Borders: Address to the Geological Section. By W. Whitaker, B.A., F.R.S., F.G.S., President of the Section.

WHEN the British Association revisits a town it is not unusual for the Sectional Presidents to refer to the addresses of their local predecessors, and to allude to the advance of their science since the former meeting. I have at all events tried to follow this course, with the sad result of having to chronicle a falling back rather than an advance in our methods of procedure; for at the meeting of 1851 all the Sectional Presidents had the wisdom not to give an address, and of all the inventions of later years I look upon the presidential address as perhaps the worst.

Had I the courage of my opinion I should not now trouble you; but an official life of over thirty-eight years has led me to do what I am told to do, and to suppress my own ideas of what is right. After all it is the fault of the Sections themselves that they should suffer the evil of addresses. They could disestablish the institution without difficulty.

On these occasions it is not usual to allude to the personal losses our science has had in the past year; but there are times when the lack of a familiar presence can hardly be passed over, and since we last met we have lost one of our most constant friends, who had served us long and well, and had been our Secretary for a far longer time than any other holder of that office. When we were at Oxford last summer none of us could have thought that it was our last meeting with William Topley.

I do not now mean to say anything on the origin or on the classification of the various divisions of the Crag and of the Drift that occur so plentifully around us, and form the staple interest of East Anglian geology. These subjects, which are the more interesting from being controversial, I leave to my brother-hammerers, and without claiming the credit of magnanimity in so doing, having said what I had to say on them in sundry Geological Survey memoirs. The object of this address is to carry you below the surface, and to point out how much our knowledge of the geology of the county in which we meet has been advanced by workers in another field, by engineers and others in their search for water. As far as possible allusion will be made only to work in Suffolk; but we must occasionally invade the neighbouring counties.

This kind of evidence has chiefly accumulated since the meeting of the Association at Ipswich in 1851; for of the 476 Suffolk wells of which an account, with some geologic information, has been published, only 68 were noticed before that year, all but two of these being in a single paper. The notes on all these wells are now to be found in twelve Geological Survey memoirs that refer to the county. Number alone, however, is not the only point, and many of the later records are marked by a precision and a detail rarely approached in the older ones. It should be stated that in the above and in the following numbers strict accuracy is not professed, nor is it material. A slight error in the number of wells, one way or the other, would make practically no difference to the general conclusions.

Now let us see how these records affect our knowledge of the various geologic formations, beginning with the newest and working downward.

The Drift.—Under this head, as a matter of convenience for the present purpose, we will include everything above the Chillesford Clay. There is no need for refinement of classification, and the thin beds that come in between that clay and the Drift in some parts do not affect the evidence we have to deal with.

As a matter of fact, it is only from wells that we can tell the thickness of the Drift over most of the great plateau that this formation chiefly forms; open sections through a great thickness of Drift, to its base, are rare, except on the coast.

There is often some doubt in classifying the beds, the division between Drift and Crag being sometimes hard to make in sections of wells and borings; but from an examination of the records of these Suffolk sections that pass through any part of the Drift Series (as defined above) we find that no less than 173 show a thickness of 50 feet and upward, whilst of these 34 prove no less than 100 feet of Drift, many reaching to much more. Of the two that are said to show a thickness of over 200 feet and the one said to be more than 300 feet deep in Drift, we can hardly feel certain; but such amounts have been recorded with certainty as occurring in the neighbouring county of Essex.

These great thicknesses (chiefly consisting of Boulder-clay) show the importance of the Drift, and the impossibility of mapping the formations beneath with any approach to accuracy, on the supposition that the Drift is stripped off, as is the case in the ordinary geologic map. The records also show the varying thickness of the Drift, and how difficult it often is therefore to estimate the thickness at a given spot. Sometimes the sections seem to point to the existence of channels filled with Drift, such as are found also in Essex and in Norfolk; and it may be noted that in the northern inland part of the former county, one of these channels has been traced, though of course not continuously, for some eleven miles along the valley of the Cam, and at one place to the depth of 340 feet (or nearly 140 below sea-level), the bottom of the Drift moreover not having been reached even then. A channel of this

sort seems to occur close to us, in the midst of the town of Ipswich, where, by St. Peter's, one boring has pierced 70 feet of Drift, and another 127, in ground but little above the sea-level.

As the Drift sands and gravels, that in many places occur below the Boulder-clay, often yield a fair amount of water, the proof of their occurrence and of the thickness of the overlying clay is of some practical good.

The Crag.—On this geologic division we have a less amount of information, as would be expected from the fact that it is not nearly so widespread as the Drift, and this information is confined to the Upper or Red Crag, the Lower or Coralline Crag occurring only over a very small area, and no evidence of its underground extension being given by wells.

What we learn of the Red Crag, however, is of interest, several wells having proved that it is far thicker underground than would have been supposed from what is seen where its base crops out. One characteristic, indeed, of this sandy deposit, in the many parts where it can be seen from top to bottom, is its thinness, as in such places it rarely reaches a thickness of 40 feet. But, on the other hand, wells at Hoxne seem to prove more than 60 feet of Crag, whilst at Saxmundham the formation is 100 feet thick, and at Leiston and Southwold over 140. Further north, just within the border of Suffolk, there is, at Beccles, a thickness of 80 feet of sand, or, with the overlying Chillesford Clay, a total of 95. Our underground information has, then, trebled the known thickness of the Upper Crag of Suffolk.

It has also shown that at some depth underground the colour-name is a misnomer, the shelly sands being light-coloured and not red. This is the case, too, with some other deposits, which owe their reddish-brown colour at the surface to peroxide of iron. Presumably the iron-salt is in a lower state of oxidation until it comes within reach of surface actions. This seems to point to the risk of taking colour as the mark of a geologic formation.

Eocene Tertiaries.—Below the Crag there is a great gap in the geologic series, and we come to some of the lower of the Tertiary formations, about which little had been published, as regards Suffolk, before the work of the Geological Survey in the county. It seems as if the special interest in the more local Crag had led observers to neglect these beds, which had been amply noticed in other parts.

We have records of more than forty wells in Suffolk that are partly in these deposits, and of these thirty-six reach down to the Chalk, twenty giving good sections from the London Clay to the Chalk. The thickness of the Lower London Tertiaries (between those formations) thus proved varies from 30 to 79½ feet, the higher figure being much greater than anything shown at the outcrop. The greatest recorded thickness is at Leiston, where, moreover, the top 26 feet of the 79½ may belong to the uppermost and most local of the three divisions of the series, the Oldhaven Beds, of very rare occurrence in the county. The next greatest thickness is at Southwold, where the whole has been classed as Reading Beds (the

persistent division), though here and elsewhere it is possible that the underlying Thanet Beds are thinly represented. It is noteworthy that at both these places, where the Lower London Tertiaries are thick, they are also at a great depth, beginning at 252½ and 218 feet respectively, which looks as if, like the Crag, they thickened in their underground course away from the outcrop.

The important evidence given by these wells, however, is not as regards thickness; it is to show the underground extent of the older Tertiary beds, beneath the great sheet of Crag and Drift that prevents them from coming to the surface north-eastward from the neighbourhood of Woodbridge. It is clear that over this large tract we can know nothing of the beds beneath the Crag otherwise than from wells and borings; and, until these were made, our older geologic maps cut off the older Tertiary beds far south of the parts to which we now know that they reach, though hidden from our sight. No one, for instance, would have imagined many years ago that at Southwold the Chalk would not be touched till a boring had reached the depth of 323 feet, or some 280 below sea-level, nor that at Leiston those figures would have been about 297 and 240.

It is from calculations based on the levels of the junction of the Chalk and the Tertiary beds in many wells that the line engraved on the Geological Survey map as the probable boundary of the latter beds under the Crag and Drift has been drawn. From what has gone before, however, as to the great irregularity in the thickness of the Drift, it is clear that this line must be taken only as approximate, and open to correction as further evidence is got; albeit the junction of the Chalk and the Tertiary beds is found to be here, as elsewhere, fairly even, along an inclined plane that sinks towards the coast.

Cretaceous Beds.—Though the *Chalk* is reached by very many wells, yet we get less information about it, by reason of its great thickness. Moreover, the great amount of overlying beds in many cases is a bar to deep exploration.

Of our Suffolk wells there are forty which go through 100 feet or more of Chalk. Of these twenty go through 200 feet or more, half of these to 300 or more, and again half of the ten to 400 or more, a very exact piece of geometric progression, or more strictly, retrogression. Although two wells pass through the great thickness of more than 800 feet of Chalk, yet neither of them gives us the full thickness of the formation; for the 816 feet at Landguard Fort do not reach to the base, whilst the 843 (or 817) feet at Combs, near Stowmarket, do not begin at the top.

As in no case yet recorded has the Chalk been pierced from top to bottom in Suffolk (a defect that will be supplied during this meeting by the description of the Stutton boring), that is to say, no boring has gone from the overlying older Tertiary beds to the underlying Gault, we must now, therefore, cross the border of the county to get full information as to the thickness of the Chalk; and we have not far to go, for the well-known Harwich boring passes through the whole of the Chalk, proving a thickness of 890 feet. It is almost

certain, indeed, that this should be given as a few feet more, for the 22 feet next beneath, which have been described as Gault mixed with Greensand, is probably in part the green clayey glauconitic base of the Chalk Marl. We may fairly add for this another 5 feet (as also in the case of the Combs boring), and may say that, in round numbers, the Chalk reaches a thickness of about 900 feet in the south-eastern part of Suffolk. Toward the northern border of the county it is probably more, as the deep boring at Norwich passes through nearly 1,160 feet of Chalk, and that without beginning at the top of the formation.

Of our recorded Suffolk wells only three reach the base of the Chalk, at Mildenhall, Culford, and Combs; consequently we have little knowledge of the divisions of the Chalk. These divisions, indeed, are of comparatively late invention, having been evolved since the publication of many of the deep sections that have been referred to.

If the Upper Chalk at Harwich goes as far down as the flints, then we must allow it to be 690 feet thick, leaving little more than 200 for the Middle and Lower Chalk together. At Landguard Fort, from the same point of view, the Upper Chalk would certainly be 500 feet thick, and one cannot say how much more.

At Combs, on the other hand, flints have been recorded as present only in the top 27 feet of the Chalk; but whilst this may have been owing in part to the boring having passed between fairly scattered nodules, and in part perhaps to insufficient care in observation, at Harwich it is possible that some flints may have been carried down in the process of boring.

What evidence we have tends to show, however, that the Upper Chalk forms a good deal more than half, and perhaps about two-thirds, of the formation, the Middle and Lower Chalk being rather thin. This agrees with what is found in other parts where the Chalk is thick, extra thickness being chiefly due to the highest division. The glauconitic marly bed at the base seems to be well developed and to be underlain by the Gault clay; so that we have no good evidence of the occurrence of the Upper Greensand. This division may be thinly represented at Mildenhall, but it is difficult to classify some of the beds passed through in the old boring there.

As far as the *Gault* is concerned little of course is known; but that little points to this formation being unusually thin, presumably only 73 feet from top to bottom at Culford, and probably not more than between 50 and 60 at and near Harwich. In the north-western part of the neighbouring county of Norfolk it is well known to be still less, the clay thinning out northward along the outcrop, until at last there is nothing but a few feet of Red Chalk between the carstone of the Lower Greensand and the Chalk. The Gault being of much greater thickness around and under other parts of the London Basin, this thinning in Norfolk and Suffolk is noteworthy. The absence of the more inconstant Upper Greensand is to be expected in most places, and calls for no remark; it may, however, be noted that geologists are coming to the conclusion that these two

divisions are really parts of one formation, and one result of this geologic wedding is for the inconstancy of one partner to be greatly compensated by the constancy of the other.

The *Lower Greensand* has been found in one deep boring only, at Culford, in the western part of the county, where it is represented by 32½ feet of somewhat exceptional beds. This slight thickness prepares us for underground thinning, and in the far east of the county the formation is presumably absent, there being no trace of it at Harwich or at Stutton.

With the Cretaceous beds we pass from the regular orderly succession of geologic formations; indeed, it may be said that when we reach the base of the Gault we pass out of the region of facts into the realm of speculation.

We have come then to perhaps the most interesting problem in the geology of the Eastern Counties, to the consideration of the question, What rocks underlie the Cretaceous beds at great depths? In dealing with this I must ask your patience for frequent excursions outside our special district, and sometimes indeed far away from it.

Beyond the outcrop of the lower beds of the Cretaceous Series in Cambridgeshire and Norfolk, we find of course a powerful development of the great Jurassic Series; but the only two recorded deep borings in and near Suffolk that have pierced through the Cretaceous base, at Culford on the north-west and at Harwich on the south-east, show not a trace of anything Jurassic: they pass suddenly from Cretaceous into far older rocks. And here a paper that is to be brought before you must be anticipated, to a slight extent, by adding that the trial-boring at Stutton shows just the same thing—the Gault resting directly on a much older rock, which cannot be classed as of Secondary age.

There is no need now to discuss the literature of the old rocks underground in South-eastern England: that has often been done. We may take the knowledge of what has been shown by the various deep borings as common property, and may use it freely, without troubling to state the source of each piece of information, and I will not therefore burden this address with references. I had indeed thought of supplementing a former account by noticing the later literature of the subject; but decided to spare you from the infliction, and myself from the trouble of inflicting; though it may be convenient to add, in the form of an appendix, a list of the chief papers on the subject that have been published since the question was discussed at length in 1889, in an official memoir on the geology of London, and to supply some omissions in that work. Nor do I propose to make any special criticism of papers on the subject that have appeared of late years; this is hardly the occasion for controversy, which may well be put off to a more convenient season. Some general remarks, however, I shall have to make after putting the facts before you.

There are 10 deep borings reaching to old rocks in the London Basin, of which accounts have been published. We find that in

4 of these (Meux's, Streatham, Richmond, and Dover) Jurassic beds separate those rocks from the Cretaceous beds; so that there are 6 in which these last rest direct on old rocks (Ware, Cheshunt, Kentish Town, Crossness, Culford, and Harwich). Stutton, of course, make a seventh. The Jurassic rocks occur only in the southern borings, either in London or still further southward, and in one case only (Dover) is there any considerable thickness of these: in the other 3 they are from $38\frac{1}{2}$ to $87\frac{1}{2}$ feet thick. As far as regards Suffolk and its borders we may therefore disregard them, except in the far west, near their outcrop, and we may pass on to consider the older rocks that have been found.

So far the occurrence, next beneath the Cretaceous or Jurassic beds, of Silurian, Devonian, and Carboniferous rocks has been proved, whilst in some cases we are still doubtful as to the age of the old rocks found. In 5 cases distinctive fossils have been found (Ware, Cheshunt, Meux's, Dover, and Harwich), but in 5 others they have not (Kentish Town, Crossness, Richmond, Streatham, and Culford), and it is in the latter group, too, that the character of the beds leaves their age in doubt. So far another must be added to these, as no fossil has yet been found in the old rocks at Stutton.

Of the above 10 deep borings in the London Basin (using that term in the widest sense, as including the Chalk tract that everywhere surrounds the Tertiary beds) we owe 9 to endeavours to get water from deep-seated rocks, and in addition to these 9 we have several other deep borings, which, though not carried through to the base of the Secondary rocks, yet give us much information concerning those beds (at Holkham, Norwich, Combs, Winkfield, London, Loughton, Chatham, and Dover). In one case only, that of Dover, has the work been done for the purpose of exploration, but now, after a few years' interval, a second trial has been made at Stutton.

Now both of these borings were started for a much more definite object than merely to prove the depth to older rocks, or the thickness of the Cretaceous and Jurassic Series. There is one particular division of those older rocks that has a distinct fascination for others than geologists. We, happily, are content to find anything and to increase our knowledge in any direction, but naturally those who are not geologists, as well as many who are, like to find something of immediate practical value. As already shown, we owe much knowledge of the underground extension of formations to explorations for water; it has now become the turn of geologists to help those who would like to find that much less general, though nearly as needful and certainly more valuable thing, *coal*.

The first place to suggest itself to those geologists who had worked at this question, as a good site for trial, was the neighbourhood of Dover, and for various good reasons. The trial has been made, and successfully, several hundred feet of Coal-measures having been found, without reaching their base, but with several beds of workable coal.

Beyond that neighbourhood, however, geologists are not in such accord, and generally speaking, fairly good reasons can be given both for and against the selection of many tracts for trial, except in and near London, where no geologists would recommend it, from the evidence in our hands.

Let us then shortly review the evidence that we have on the underground extension of the older rocks in South-eastern England, with a view of considering the question of the possibility of finding Coal-measures in any of the folds into which those rocks have probably, nay almost certainly, been thrown.

The area within which the borings that reach older rocks in the London Basin is enclosed is an irregular pentagon, from near Dover, on the south-east, to Richmond on the west, thence to Ware, thence to Culford on the north, thence to Harwich, and thence southward to Dover, the greatest distance between any borings being from Dover to Culford, about eighty-six miles. It is therefore over a large tract, extending, of course, beyond the boundaries sketched above, that we have good reason to infer that older rocks are within reasonable distance of the surface, nowhere probably as much as 1,600 feet, and mostly a good deal less.

We must now consider some evidence outside the tract hitherto dealt with. Southward of the central and eastern parts of the London Basin we have evidence that the Lower Cretaceous beds thicken greatly, from what is seen over their broad outcrop between the North and South Downs. We know also, from the Dover and Chatham borings, that the Upper and Middle Jurassic beds come in to the south-east, whilst the Sub-Wealden Exploration, near Battle, proves that those divisions thicken greatly southward, the latter not having been bottomed at the depth of over 1,900 feet at that trial-boring.

Westward, however, near Burford in Oxfordshire, and some miles northward of the nearest part of the London Basin, Carboniferous rocks have been found at the depth of about 1,180 feet, these being separated from the thick Jurassic beds (including therein the Liassic and Rhætic) by perhaps 420 of Trias. They consist of Coal-measures, which were pierced to the depth of about 230 feet.

In and near Northampton, north-eastward of the last site, and still further from the northern edge of the London Basin, the like occurs; but the beds found are older than the Coal-measures, and the Trias is thin, not reaching indeed to 90 feet in thickness, and being absent in one case. At one place, too, the Carboniferous beds have been pierced through, with a thickness of only 222 feet, when Old Red Sandstone was found, and in another place still older rock seems to have been found next beneath the Trias. The depth to the rocks older than the Trias, where they were reached, was 677, 738, and 790 feet, or respectively 395, 460, and 316 below sea-level. Some of these figures must be taken as somewhat approximate, though they are near enough to the truth for practical purposes.

A boring at Bletchley, to the south, reached granitic rocks at the

depths of 378½ and 401 feet; but these rocks seem to be only boulders in a Jurassic clay: their occurrence, however, is suggestive of the presence of older rocks at the surface no great way off, in Middle Jurassic times.

Much further northward, at Scarle, south-west of Lincoln, the older rocks have been reached at the depth of about 1,500 feet, all but 141 of which are Trias, and they begin with the Permian (which crops out some eighteen miles westward), the Carboniferous occurring after another 400 feet, and having been pierced to 130.

We have then evidence that over a large part of South-eastern England, reaching northward and westward of the London Basin, though the older rocks are hidden by a thick mantle of Jurassic, Cretaceous, and Tertiary beds, yet they seem to be rarely at a depth that would be called very great by the coal-miner. They are distinctly within workable depths wherever they have been reached.

There is no area of old rocks at the surface in our island, south of the Forth, in which Coal-measures are not a constituent formation. Truly, further north, in the great tract of Central and Northern Scotland there are no Carboniferous rocks; but we can hardly say that none ever occurred, at all events in the more southern parts. We know, though, that on the west and north Jurassic and Triassic beds rest on formations older than the Carboniferous.

It is not, however, to this more northern and distant tract that we should look for analogy to our underground plain of old rocks; rather should we look to more southern parts, to Wales and to Central and Northern England, where Coal-measures are of frequent occurrence. On the principle of reasoning from the known to the unknown, I cannot see why we should expect anything but a like occurrence of Coal-measures, in detached basins, in our vast underground tract of old rocks.

What, then, is the evident conclusion from what we know and from what we may reasonably infer? Surely that trials should be made to see if such hidden coal-basins can be found.

One trial has been made, and it has succeeded: the Dover boring has proved the presence of coal underground in Eastern Kent, along the line between the coal-fields of South Wales and of Bristol on the west, and those of Northern France and of Belgium on the east.

The long gap between the distant outcrops of the Coal-measures near Bristol and Calais has been lessened very slightly by the working of coal under the Triassic and Jurassic beds near the former place, but much more by our brethren across the narrow sea, the extent of the Coal-measures beneath the Jurassic and Cretaceous beds, having not only been proved by the French and the Belgians along their borders, but the coal having been largely worked. At last, we too have still further decreased the gap, by the Dover boring, a work that I trust is to be followed by other work along the same line.

But is this the only line along which we are to search? Are we to conclude that the only coal-fields under our great tract of

Cretaceous beds (where these are either at the surface or covered by Tertiary beds) are in Kent, Surrey, and other counties to the west? Have we no coal-fields but those of Bristol and of South Wales? The bounds of our midland and northern coal-fields have been extended by exploration beneath the New Red Series: are we to stop here and to assume that there can be no further underground extension of the Coal-measures south-eastward? This seems hardly a wise course, and is certainly a very unenterprising one. It seems to me rather that the right thing to be done is to try to find out the real state of things, by means of borings.

There are, of course, objectors in this as in other matters. Some may say that it is silly to try in Suffolk, and that Essex gives a better chance of success. Others, again, may prefer Norfolk. And yet others may argue that there is no chance of finding Coal-measures in any of those three counties. But I must confess my inability to understand this line of reasoning; the fact is, that the data we have are few and far between, and that we want more. It is really of little use to bandy words, and I do not now mean to take up the matter in detail. We cannot get at the truth except by actual work; justification by faith will not hold in this case, still less justification by unfaith.

Let us hark back a little and call to mind what has happened in the past. I remember the time when certain geologists disbelieved in the possibility of the occurrence of Coal-measures anywhere in South-eastern England, it being argued that the formation thinned out before it could get so far eastward. Then this view was somewhat varied, and it was inferred, from certain observed facts, that even if Coal-measures did reach underground into these benighted parts, they would be without workable coal, and so practically useless.

Now for some years nothing occurred to upset the prophets of evil, that is to say, no fact came to light. There were not wanting inferences to the contrary, but it remained practically a matter of opinion. One day, however, the needful fact came, and the first boring made specially to test the question (at Dover) disproved both the above negative theories by finding Coal-measures with workable coal. Let us hope that a like result may happen in East Anglia, and that the pessimists may again be in the wrong.

We should not, however, fall into the opposite error, that of optimism. We must not expect an immediate success like that at Dover. We are here much further from any known coal-field. Advertisements of various wares sometimes tell us that "one trial will suffice," but it is not so in this case. We should not be content until many borings have been made, and we should not be despondent if, after sites have been selected to the best of our judgment, we begin with a set of borings that are unsuccessful in finding coal.

At the time of writing I cannot say that the Stutton boring is a success or a failure as far as coal is concerned, but I am quite ready to accept the latter without being discouraged. Whatever it is you

may know during our meeting; it is certainly a success in the matter of reaching the old rocks at a depth of less than 1,000 feet. We should remember that every boring is almost certain to give us some knowledge that may help in future work.

There is a further point, however, to be taken into account. A boring that may at first seem to be a failure, from striking beds older than the Coal-measures, may some day turn out otherwise. The coal-field along the borders of France and Belgium is sometimes affected by powerful and peculiar disturbances, by faults of comparatively gentle inclination (far removed from the usual more or less vertical displacements) which have thrown Coal-measures beneath older beds in large tracts. This is no mere theory, though advanced as such at first by some Continental geologists, who have had the great satisfaction of seeing their theory adopted by practical men, and proved to be true, much coal being worked below the older beds that have been pushed above the Coal-measures by overthrust faults.

Our trial work, of course, does not yet lead us to consider such disturbances as those alluded to. We have at first to assume a normal succession of formations, and not to carry on explorations in beds that can be proved to be older than the Coal-measures; but the time may come when it will be otherwise.

Another matter to which attention has been drawn by our foreign friends is an apparent general persistence of disturbances along certain lines, or, in other words, the recurrence of disturbances in newer beds in those parts where earlier movements had affected older beds; so that, reasoning backward, where we see marked signs of disturbance for long distances in beds at or near the surface, there we may expect to find pre-existing disturbances of the older beds beneath. This, however, is a somewhat controversial question, and much remains to be done on it; but should it be proved as a general rule it may have much effect on our underground coal.

Finally, the question of the possibility of finding and of working coal in various parts of South-eastern England is not merely of local interest; it is of national importance. The time must come when the coal-fields that we have worked for years will be more or less exhausted, and we ought certainly to look out ahead for others, so as to be ready for the lessening yield of those that have served us so well. It is on our coal that our national prosperity largely, one may say chiefly, depends, and, as far as we can see, will depend. Let us not neglect any of the bounteous gifts of nature, but let us show rather that we are ready to search for the treasures that may be hidden under our feet, and the finding of which will result in the continued welfare of our native land.

ABSTRACTS OF PAPERS READ AT THE BRITISH ASSOCIATION MEETING, IPSWICH, SEPTEMBER 1895.

III.—SOME REMARKS ON THE STEREORNITHES, A GROUP OF EXTINCT BIRDS OF PATAGONIA. By C. W. ANDREWS, B.A., B.Sc., F.G.S., Assistant in the British Museum.

THE history of the discovery of the extinct birds of Patagonia is briefly given, and the more important of the opinions that have been expressed as to their affinities are noticed.

The age of the deposits in which the remains occur is probably much later than the Eocene, to which they are usually referred, and may perhaps be taken as Miocene.

The structure of the skull and skeleton of *Phororhacos*, as described by Ameghino in his recent valuable and interesting paper, is discussed and compared with that of some other birds. In the absence of actual specimens it is impossible to arrive at any very definite results, but it may be suggested that some, at least, of the Stereornithes may be related to the Geniformes, particularly to the Dicholophi. Others of the group are imperfectly known, but some of them appear to differ considerably from *Phororhacos*. It therefore seems probable that the "Stereornithes" may include a somewhat heterogeneous collection of birds which have lost their power of flight in consequence of some local conditions affecting their mode of life, and in correlation with the reduction of their wings attained a greatly increased size. A similar instance is to be found in the extinct birds of New Zealand, where the Dinornithid *Apteryx*, *Cnemiornis*, *Aptornis*, etc., all flightless, and for the most part of great size, formerly coexisted. In most cases, as soon as the peculiar conditions to which they are adapted pass away, such specialized forms become extinct, and this appears to have happened to the Stereornithes.

There seems no reason why such groups of flightless birds should not arise at any period and in any region, providing the conditions of life are favourable; indeed, the Gastornithidæ, in the Eocene of Europe, and the Stereornithes, in somewhat later deposits in South America, may be taken as instances of this.

In the Stereornithes the keel of the sternum was no doubt reduced or absent, so that they were "Ratites" in the narrowest sense of the word; but on the other hand, they can hardly be referred to the sub-class Ratitæ as usually understood, the members of which possess numerous primitive characters which point to the conclusion that they are the survivors of a group, or perhaps several groups, not necessarily contemporaneous, of ancient and generalized birds in which the power of flight had been lost, perhaps even in the Secondary period. Unfortunately, the want of any means of distinguishing truly primitive characters from those which Fürbringer calls "pseudo-primitive," which are acquired in the course of retrogression (*rückbildung*), makes it impossible to determine the exact relation of the "Ratitæ" to other flightless birds, and until a long series of remains from different horizons is available, this uncertainty must remain. It seems very doubtful whether *Gastornis* is at all related to the South American forms.

IV.—The Cladodonts of the Upper Devonian of Ohio. By Professor E. W. Claypole, D.Sc. (Lond.).

NUMEROUS specimens of the Cladodonts of the Cleveland Shale in Ohio have been found by Dr. William Clark. They for the first time reveal to us the general form of the fishes to which belonged the teeth that have alone so long represented the genus *Cladodus*. The fossils are in very fair preservation, but their state of pyritization has obscured many of the details of their structure. So far as regards their form, however, we now know that they were long, slender fishes, resembling in their character the sharks of the present day; that they possessed well-developed and powerful pectoral and caudal, with weak ventral fins, the dorsals being unknown; that they were for the most part, or altogether, spineless; that at least one species possessed cladodont teeth of more than one pattern; and that they had near the hind end of the body a peculiar flat expansion or membrane of rudely semicircular form, which gave to the caudal extremity when seen from above the outline of a sharp-pointed shovel.

The largest whole specimen yet found shows a fish of about 6 feet in length, but detached teeth and other fragments indicate others of double this size, and supply abundant proof that in late Devonian times, and in the North American area, the elasmobranch fishes had attained very great proportions and a high stage of development.

Hitherto the Cladodonts have been regarded as, in the main, characterizing the Lower Carboniferous rocks, but we now find them abounding in the earlier Devonian strata, and, as shown by the contents of their stomachs, preying—in some cases at least—on the smaller placoderms of the same area.

From the evidence of the new specimens it appears most likely that the species already defined from single and isolated teeth can no longer be maintained.

For details see the papers in the "American Geologist" for 1893-4-5.

V.—The Great Devonian Placoderms of Ohio, with Specimens. By Professor E. W. Claypole, D.Sc. (Lond.).

THE Upper Devonian Shales of Ohio have recently afforded a remarkable series of fossil fishes rivalling in size and interest those found many years ago in the Old Red Sandstones of similar age in Scotland, and described by Agassiz and Hugh Miller. The earliest of these, *Dinichthys*, was closely studied, and its structure was well explained by the late Dr. Newberry. It was an immense armour-clad fish, whose head measured from 2 to 3 feet in length. *Titanichthys*, the second of the group, though less massive, was of yet larger size. *Gorgonichthys*, the third, was described by the present writer in 1893, and, so far as is yet known, was the most formidable of all, possessing jaws of enormous size and thickness, above 24 inches long, ending in teeth or points from 6 to 9 inches in length. The fourth and last, *Brontichthys*, of which a description was also published by the writer in the "American Geologist" for 1894, is equally heavy and of equal size, but differs from all the rest

in possessing very massive symphysial portions in the mandibles with sockets apparently for the reception of teeth, as in *Titanichthys*.

Of the two last-named genera only the jaws are yet known with exactness. Other portions have been found of *Gorgonichthys*, but are still embedded in the matrix. So far as can at present be determined, all the four are closely allied to *Coccosteus*, and belong to the same family.

The set of casts exhibited in illustration of the fossils has been prepared by their discoverer, Dr. William Clark, and faithfully represents the originals, of many of which only single specimens are yet known. The labour of extricating them from the pyritous shale has proved very heavy, and much yet remains to be done in this direction.

VI.—Zonal Division of the Carboniferous System. By E. J. Garwood, M.A., F.G.S., and J. E. Marr, M.A., F.R.S.

THE authors call attention to previous attempts which have been made to divide the Carboniferous rocks into zones, noting the zonal divisions of the Lower Carboniferous rocks of North England established by De Koninck and Lohest, and the view expressed by Waagen that fuller work will enable geologists to define a series of zones in the Carboniferous as in older and newer strata.

The detailed work of one of the authors (Mr. Garwood) leads them to suppose that the following zones occur in the Lower Carboniferous beds of the northern part of the Pennine Chain and adjoining regions :—

Zone of Productus *cf.* Edelburgensis.
„ P. latissimus.
„ P. giganteus.
„ Chonetes papilionacea.
„ Spirifera octoplicata.

Mr. Garwood has traced the zone of *Productus latissimus* occupying the same relative position to that of *P. giganteus*, from Settle, in Yorkshire, to the Northumbrian coast, near Howick Burn.

The authors believe that brachiopods and goniatites will furnish good results, if a detailed study of their distribution be made; and they suggest that a committee be appointed to inquire into the possibility of dividing the Carboniferous rocks into zones, to call the attention of local observers to the desirability of collecting fossils with this view, and, if possible, to retain the services of eminent specialists, to whom these fossils may be submitted.

VII.—Probable Extension of the Seas during Upper Tertiary Times in Western Europe. By G. F. Dollfus.

TAKING into consideration the position and nature of all the outliers of Upper Tertiary age, the author is led to the following conclusions as to the extension of the Neogenic seas in Western Europe. During Miocene times England was united to France, and we have proof of the existence of two seas in the western part of Europe: one on the east extended over part of Belgium (Bolderian system), Holland, and north of Germany—probably

this sea was not very far off the eastern coast of England; the other sea, the Western, or old Atlantic Sea, was off Ireland, penetrating in various gulfs into France, as in some part of Contentin, Brittany, in the Loire valley, in the gulf of the Gironde, but there was no way of communication with the Mediterranean basin crossing France. In North Spain there are no Miocene deposits, in Portugal Miocene beds are purely littoral.

The communication with the Mediterranean Sea was certainly by the valley of the Guadalquivir. The Gibraltar Strait had not exactly its present place. The fauna of these Miocene coasts was warm and very similar to the existing fauna of Senegal and Guinea.

We can divide Pliocene time into three periods, but the situations of the seas were not very different. England was always in direct continental communication with France; the English Channel was not open at all. All the Pliocene deposits of Belgium, North France, or England, even the Lenham beds, are on the side of the North-eastern Sea; we find all these patches on the northern side of the great anticlinal line of the Artois, Boulonnais, and Weald. The fauna is different from the Miocene, and colder—it even turns more and more cold during the progress of Pliocene time. On the western or Atlantic side we have little gulfs leading the sea into the land, but not so frequently and not so far as during Miocene times. The Cornwall deposits, Contentin beds, and the Brittany patches are very limited; the basin of the Gironde contains no trace of Pliocene beds, and we have no trace of recent marine beds at the foot of the Pyrenees. In the north of Spain there is also no trace of Pliocene beds. The continent seems to have been higher, and the Atlantic tolerably distant. All the Portuguese sands recently discovered are littoral, and only on the Algarve coast and south of Spain do we find proof of the probable communication with the Mediterranean. The Gibraltar Strait was not always in the same place during Pliocene time; in the beginning probably the Guadalquivir valley to Murcia continued to be the strait, but later the rock of Gibraltar was separated from Africa and a new road was open; this way was certainly deeper than the former one, and as deep as the existing strait. By this depression the cold fauna of the depths of the Atlantic penetrated into the Mediterranean Sea as far as Sicily and Italy with *Cyprina islandica*.

The geology of Morocco is unknown, but we have plenty of information on Algeria. We have there great Miocene deposits raised along the Atlas Chain up to a great altitude, and a little lower a good and very long band of Pliocene beds of marine and continental origin. Quaternary deposits, similarly continental and littoral, occur lying along the actual coast, pointing out the south side of the Mediterranean connection.

In a few words, the English Channel has been opened very recently, and no sea occupied its place before. No sea has crossed France or Central Spain, and we are obliged to seek for an outlet for the Eastern Sea during Miocene time by way of Germany, Galicia, and South Russia, or by the north of Scotland.

During the existence of the Pliocene seas there was no other

communication for the Crag seas than the northern one, for the western, the southern, and eastern sides were undoubtedly shut in by land.

VIII.—On the Importance of Extending the Work of the Geological Survey of Great Britain to the Deep-seated Rocks, by Means of Boring. By F. W. Harmer, F.G.S.

THE systematic exploration of the subterranean geology of these islands is equally important from a scientific and a practical point of view. At present our knowledge of the structure of the rocks which form the foundation of our island home is due either to isolated and occasional borings, such as that of the Ipswich Syndicate in search of coal, or to deep wells sunk by mercantile firms; but the latter do not reach further than is necessary to obtain a supply of water, and the work is generally suspended just where it becomes geologically most interesting. But such a Survey is important practically, because unsuspected sources of wealth may be hidden under our very feet.

It is a mistake to suppose that a discovery such as that of a new coal-field would enrich only the landowners of the district, because whenever any appreciation of real property takes place, the State at once claims its share of the increased value, both for imperial and local purposes. The average for the whole country of the rates raised by local taxation alone was, for 1891, 3*s.* 8*d.* in the pound, to which must be added imperial taxes and the tithe. It may be stated roughly, that for every £100 of yearly 'unearned increment' the State is benefited in one way or another by £25, or one-fourth of the amount. The discovery of a new coal-field would cause increased prosperity in the district in which it occurred, and from this the State, through taxation, would derive great though indirect advantage.

The growing difficulty of finding employment for the ever-increasing population of these islands is a strong reason why this Survey should be undertaken.

Part of the cost might be borne by the landowners under whose property any minerals were discovered. Certain districts should be selected with the consent of the Local Authorities, and Parliamentary power taken to charge a royalty on any minerals obtained below a certain depth. Landowners would probably welcome proposals to make borings on their estates on such conditions. In the first instance, however, the Survey should map out accurately the subterranean limits of existing coal-fields, or mineral-bearing rocks, but trial borings should be put down in different localities, and each new boring would help to show more plainly the direction in which further investigations should be made. Much light would be thrown by such a Survey on the circulation of underground waters, a matter of great practical importance.

The expense of boring would be much reduced if undertaken on a large scale, as machinery and apparatus would be available again and again. The Survey would employ its own workmen, who would become increasingly efficient and economical.

REVIEWS.

OPEN-AIR STUDIES: AN INTRODUCTION TO GEOLOGY OUT-OF-DOORS. By GRENVILLE A. J. COLE, M.R.I.A., F.G.S., Professor of Geology in the Royal College of Science for Ireland, Dublin. London: Charles Griffin and Co. (Limited), Exeter Street, Strand, 1895. 8vo, pp. xii and 322, with Eleven full-page Illustrations, and Thirty-three Illustrations in text.

PROFESSOR COLE has done excellent service to our science if only by pointing out to his readers that geology is a study for the *open air*, and can no more be acquired satisfactorily in a museum or a laboratory than can the study of biology. We are reminded of a story told of the Swedish Botanist Solander, who had looked for so long a time at plants in the Museum *Herbarium*, that when a green living plant was brought to him by a lady to be named, he studied it attentively and then said: "Madam, if you shall take this plant home and put it between paper and shall sit upon it for a week, I shall tell you its name."

It is by a visit to a quarry by the roadside, or to the cliffs on the seashore, that boys may be stimulated to become geologists; and it is doubtless due to the inveterate love of bird's-nesting among country schoolboys (and all schools ought to be in the country) that we owe the passion for ornithology which has so long prevailed amongst us, and is still a dearly-loved pursuit of Englishmen.

While, however, the love for Natural History and the passion for enquiry are usually born, and must always be nurtured in the field, yet we are compelled to admit that no progress can be made without the aid of books and teachers; and that those who seek not only to acquire, but to advance, knowledge relating to rocks or fossils, must now-a-days prepare themselves by a course of study in a petrological or biological laboratory. Such training is as needful to the geologist as it is to the engineer or the surgeon.

We would not deter those living in the country, who have no means of special training, from giving attention to geology. Regarding it as a recreative science they may do useful work in collecting fossils and recording sections of the strata; but one chief lesson which all of us have to learn is how far our own knowledge can be relied upon, and when to depend upon others for the identification of minerals, rocks, and fossils. Wonder and interest may be aroused by personal observation among the rocks; but the lessons which can be learnt in the field will not be fruitful without close attention to the literature of the subject and to present methods of research.

Professor Cole recognizes the need of all this, and although he explains as many technical terms as he can in the field, he gives us a severe introductory chapter on the materials which form the earth's crust, before he asks us to accompany him out-of-doors. In a work dealing with so complex a subject as geology it is impossible to explain every term that is used; but this account of rocks and minerals, and elements, will show the student the kind

of fundamental knowledge he must acquire. This very chapter is one that may repel a general reader whose enthusiasm has not already been kindled, and who desires only to know the leading objects and conclusions of geological enquiry; but as no sound knowledge can be acquired without application, the earnest student will do well to master this preliminary lesson (with the aid of specimens) before he engages in the more inviting open-air studies.

Once in the field the student is led on to "a mountain hollow" to witness the work of glaciers, and "down the valley" to examine the work of springs and rivers. He is taken more or less rapidly from one country to another "along the seashore" and "across the plains." He passes from the "dead volcanoes" of Auvergne and other regions to "a granite highland," and then to the volcanic mountains of Skye. He is given a rapid and consequently meagre account of "the Annals of the Earth," of its main epochs, and their rocks and fossils. Then "the Surrey Hills" are visited, and many particulars are given of the Cretaceous rocks, as a sample of what might be said with respect to other strata and their fossils.

The author introduces a table showing the foreign divisions with which some of our English formations are correlated; but such names as *Urgonian* for Atherfield Clay, and *Cenomanian* for Upper Greensand and Lower Chalk, if used at all in this country, are for the Museum and not for the Field. In their broad usage they apply to minor epochs of time rather than to formations; and the author rightly employs the local stratigraphical terms in his field-lessons. But why separate Gault from Upper Greensand in so marked a way as to put the former into Lower and the latter into Upper Cretaceous?

A study of the Weald leads on to a study of "the folds of mountains," and to lessons on the results of earth-movements and attendant phenomena.

The nine chapters dealing with open-air studies thus cover a good deal of ground, and form a fitting introduction to the principles of geology. Writing in a pleasant and familiar style, that reminds us not a little of "The Gamekeeper at Home," the author shows himself so enthusiastic a lover of Nature that his work cannot fail to arouse keen interest in geology. We would commend it to all beginners, and to the "Home Reading Union," for although there may be a little too much of detail, here and there, to please everybody, yet the explanations are lucid, and the narrative is for the most part attractive enough, even if it be discursive. We would commend it also to advanced students and teachers, for they would surely freshen up their knowledge on many a subject, whether it be on salt-lakes or volcanic phenomena, chalk-flints, or pressure-metamorphism. The beginner, let us hope, after reading this book, will be tempted to proceed to more systematic works, and although in the fuller records he will find the facts not embedded in quite so picturesque a matrix, yet the fascinating Open-air Studies of Prof. Cole will have given the subject a glow of animation, which will be fostered and developed by actual work in the field.

OBITUARY.

THOMAS JAMES SLATTER, F.G.S.

Born 1834. Died August 1st, 1895.

Thomas James Slatter, F.G.S., whose decease we have now to report, died at his house, The Drift, Evesham, on the 1st of August. He was a geologist whose knowledge of the locality in which he lived and worked was most intimate and reliable. He was born in Gloucester in 1834, but his family was for many years located at Stratton, near Cirencester. He was the cousin and intimate friend of John Jones, of Gloucester, whose contributions to pages of the earlier numbers of this Magazine, and to the Proceedings of the Cotteswold Naturalists Club, were well known. Mr. Slatter commenced his business life as quite a young man in the Gloucestershire Bank, and then took up his abode in Evesham. He became successively manager of the Moreton-in-Marsh, Redditch, and Evesham branches of the Bank, but retired into private life a few years since, and, having built a new house on Green Hill, near the latter town, removed into it his extensive and most interesting collection of fossils. He was elected a Fellow of the Geological Society of London in 1879, but, to the regret of those who knew how careful he was as an observer, he was never the author of any work on geology, nor even of any contribution to a periodical on the geology of the district he knew so well. His death, by paralysis, took place in the house which he had so lately erected.

R. F. T.

JAMES CARTER, F.R.C.S., F.G.S.

Born October 3rd, 1813. Died August 31st, 1895.

We regret to record the death of our old and valued friend, and fellow-worker in fossil Crustacea, Mr. James Carter, F.R.C.S., F.G.S., of Cambridge, in his eighty-second year. During the greater part of his life Mr. Carter practised as a surgeon in Cambridge, where his house, in Petty Cury, was for many years the resort of the leading geologists and men of science in the University, who never failed to find in Mr. and Mrs. Carter genial, cultivated, and hospitable hosts.

He was especially interested in palæontology, and devoted much of his time to this and other scientific subjects. He contributed papers to the Geological Magazine and the Quarterly Journal of the Geological Society, the chief being "On a New Species of *Ichthyosaurus* from the Chalk" (1846), "On *Orithopsis Bonneyi*" (1872), "On a Skull of *Bos primigenius* perforated by a Stone Celt" (1874), "On the Decapod Crustaceans of the Oxford Clay" (1886), and "On Fossil Isopods, with a Description of a New Species" (1889).

Mr. Carter was recognized as an authority on the fossil Decapod Crustacea; for some time he has been engaged in collecting materials for a monograph on that group, and has left his manuscript in an advanced state. He retained his interest in his pursuits almost till the last, and was engaged in his scientific work to within a few weeks of his death. He served on the Councils of the

Geological and Palæontographical Societies for some years, and was a local secretary of the latter society. Mr. Carter presented his collection of Cambridge fossils to the Woodwardian Museum some years before his death.—(*H. W.* and *Athenæum.*)

PROFESSOR SVEN LOVÉN,
OF STOCKHOLM.

BORN JANUARY 6TH, 1809. DIED SEPTEMBER 3RD, 1895.

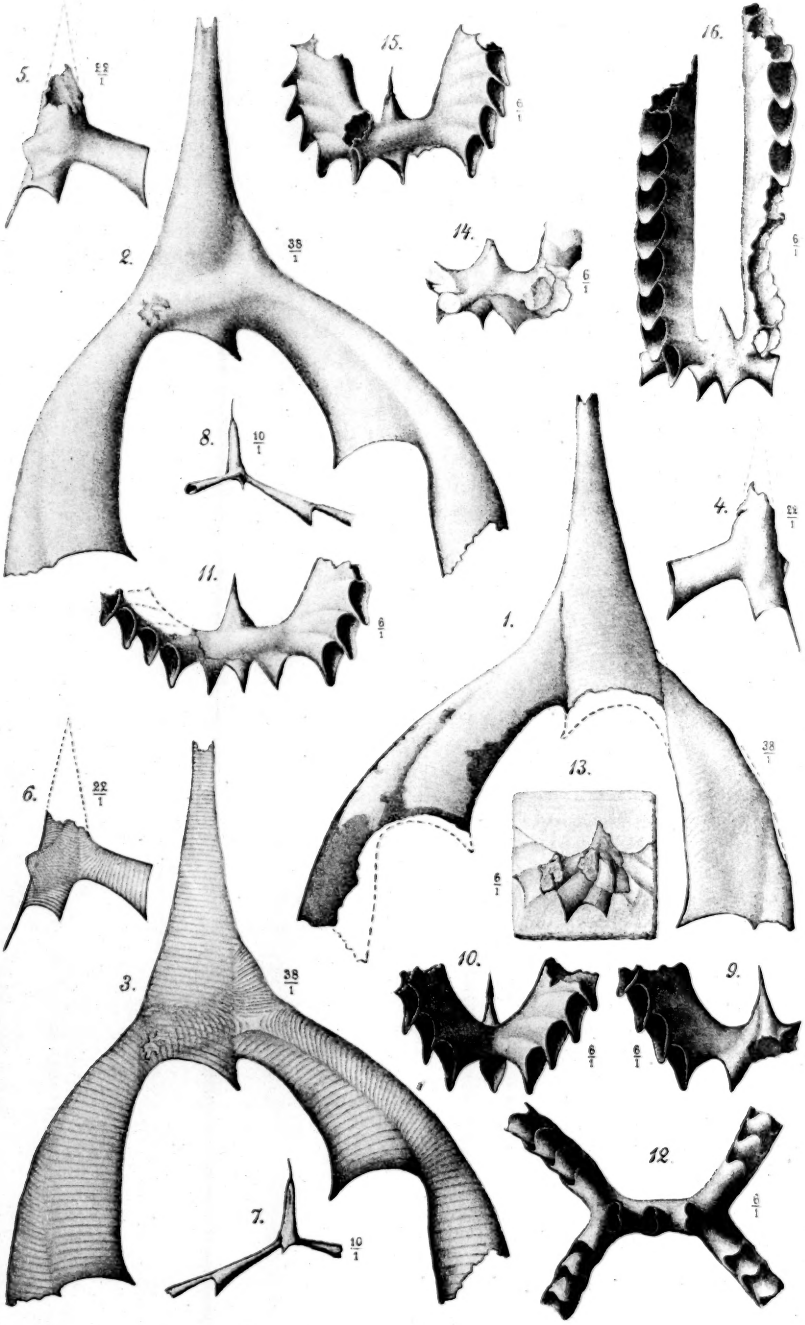
ANOTHER distinguished biologist has just passed away, one out of a number of those whose births have made famous the beginning of this century, now so nearly expired. Lovén of Stockholm may well take rank with Owen of London, Milne-Edwards of Paris, Siebold of Munich, and Van Beneden of Louvain, as one of the great pioneers of natural science.

Lovén was born at Stockholm in 1809, and matriculated at Upsala, taking the degree of Doctor of Philosophy in the University of Lund. In 1830 he became a Docent of the University, and, after having attended the lectures of Ehrenberg and Ritter at Berlin, he devoted himself to the study of the marine fauna of the shores of Scandinavia, explored the Baltic and North Seas, and conducted the first scientific expedition to Spitzbergen in 1837. Lovén was elected Professor of Geology and Keeper of the Invertebrata in the Natural History Museum in Stockholm in 1841. In 1868 the University of Lund conferred upon him the degree of M.D. *honoris causa.*

As early as 1835 we find him writing upon the hydroid zoophytes; in 1846 he published an Index of the Mollusca of the Western Shores of Scandinavia, and devoted himself to the study of the development of mollusca. Later on in life he made a minute examination of the structure of Echinoderms, on which he published several beautifully illustrated memoirs.

Writing at first in Swedish or in German, he gave, later on, his results in French, but for the closing years of his laborious life he wrote in English. As the chief of an important department of the Zoological Museum at Stockholm, he brought his exhibited specimens to a high degree of artistic beauty, and it was in virtue of this position, and not because of any connection with a University, that he was, *secundum mores Scaniæ*, known as Professor.

His researches on the anatomy and physiology and the geographical distribution of the marine invertebrata, gained for him election as a Member of the Academy of Stockholm in 1840. He was made a Corresponding Member of the Institute of France in 1872; a Foreign Member of the Geological Society of London in 1882, and of the Royal Society of London in 1885. He resigned his connection with the Museum a few years since, the burden of bodily pains being too great for his advancing years, rendered sad by the early death of a son of great promise. Those, however, who had been brought into personal contact with him felt that so long as Lovén lived they had a real friend. His charming geniality and his remarkable kindness to men much younger than himself made a deep and lasting impression on all who had the pleasure to know him personally.—(In part from the *Athenæum,* Sept. 14th, 1895.)



Ljustryck af Chr. Westphal Stockholm.